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future historians of ancient Israel.'* HANS M. BARSTAD

LESTER L. GRABBE

ANCIENT ISRAEL

WHAT DO WE KNOW AND HOW DO WE KNOW IT?



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What Do We Know and How Do We Know It?

by
Lester L. Grabbe



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Dedicated to

Professor Loren R. Fisher

The European Seminar in Historical Methodology

My History of Israel students over a quarter of a century

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Abbreviations

AASOR	Annual of the American Schools of Oriental Research
AB	Anchor Bible
ABD	David Noel Freedman (ed.), <i>Anchor Bible Dictionary</i> (6 vols; Garden City, NY: Doubleday, 1992)
ACEBT	<i>Amsterdamse Cahiers voor Exegese en bijbelse Theologie</i>
ADPV	Abhandlungen des Deutschen Palästina-Vereins
AEL	M. Lichtheim (ed.), (1971–80) <i>Ancient Egyptian Literature</i> (3 vols; Berkeley)
AfO	<i>Archiv für Orientforschung</i>
AGAJU	Arbeiten zur Geschichte des antiken Judentums und des Urchristentums
AHI	Graham I. Davies, <i>Ancient Hebrew Inscriptions: Corpus and Concordance</i> (Cambridge University Press, 1991)
AJA	<i>American Journal of Archaeology</i>
ALASPM	Abhandlungen zur Literature Alt-Syren-Palästinas und Mesopotamiens
ALGHJ	Arbeiten zur Literatur und Geschichte des hellenistischen Judentums
AnBib	Analecta biblica
ANET	James B. Pritchard, <i>Ancient Near Eastern Texts Relating to the Old Testament</i> (3rd ed. with Supplement; Princeton: Princeton University Press, 1969)
AOAT	Alter Orient und Altes Testament
AP	Cowley, A., (1923) <i>Aramaic Papyri of the Fifth Century B.C.</i> (reprinted Osnabruck: Otto Zeller, 1967).
ARAB	Daniel David Luckenbill, <i>Ancient Records of Assyria and</i>

Abbreviations

	<i>Babylonia</i> (4 vols; 1926–27; reprinted London: Histories and Mysteries of Man, 1989)
ASOR	American Schools of Oriental Research
ATD	Das Alte Testament Deutsch
AUSS	<i>Andrews University Seminary Studies</i>
BA	<i>Biblical Archeologist</i>
BAR	<i>Biblical Archaeology Review</i>
BASOR	<i>Bulletin of the American Schools of Oriental Research</i>
BBB	Bonner biblische Beiträge
BCE	Before the Common Era (= BC)
BETL	Bibliotheca Ephemeridum Theologicarum Lovaniensium
BHS	<i>Biblia Hebraica Stuttgartensis</i>
<i>Bib</i>	<i>Biblica</i>
BibOr	Biblica et orientalia
BIS	Biblical Interpretation Series
BJRL	<i>Bulletin of the John Rylands Library</i>
BN	<i>Biblische Notizen</i>
BO	<i>Bibliotheca Orientalis</i>
BWANT	Beiträge zur Wissenschaft vom Alten und Neuen Testament
BZ	<i>Biblische Zeitschrift</i>
BZAW	Beihefte zur ZAW
CAH	<i>Cambridge Ancient History</i>
CBC	Century Bible Commentary
CBET	Contributions to Biblical Exegesis and Theology
CBQ	<i>Catholic Biblical Quarterly</i>
CBQMS	<i>Catholic Biblical Quarterly Monograph Series</i>
CBR	<i>Currents in Biblical Research</i>
CIS	<i>Corpus Inscriptionum semiticarum</i>
CML	J. C. L. Gibson, <i>Canaanite Myths and Legends</i> (1978)
ConBOT	Conjectanea biblica, Old Testament
CoS	Hallo, William W. (ed.), (1997–2002) <i>The Context of Scripture</i> , (3 vols).
CRAIBL	Comptes rendus de l'Académie des inscriptions et belles-lettres
CRBS	<i>Currents in Research: Biblical Studies</i>
DDD	K. van der Toorn, B. Becking, and P. W. van der Horst (eds) <i>Dictionary of Deities and Demons in the Bible</i> (1999)
DSD	<i>Dead Sea Discoveries</i>
DtrH	Deuteronomistic History
EA	el-Amarna letters

Abbreviations

EB	early Bronze Age
EI	<i>Eretz-Israel</i>
ESHM	European Seminar in Historical Methodology
ET	English translation
EvT	<i>Evangelische Theologie</i>
FAT	Forschungen zum Alten Testament
FOTL	Forms of Old Testament Literature
FRLANT	Forschungen zur Religion und Literatur des Alten und Neuen Testaments
FS	Festschrift
HAT	Handbuch zum Alten Testament
HdA	Handbuch der Archäologie
HSM	Harvard Semitic Monographs
HSS	Harvard Semitic Studies
HTR	<i>Harvard Theological Review</i>
HUCA	<i>Hebrew Union College Annual</i>
ICC	International Critical Commentary
IDB	G. A. Buttrick (ed.) <i>Interpreter's Dictionary of the Bible</i> (4 vols; Nashville: Abingdon, 1962)
IDBSup	Supplementary volume to IDB (1976)
IEJ	<i>Israel Exploration Journal</i>
IOS	Israel Oriental Society
JANES	<i>Journal of the Ancient Near Eastern Society of Columbia University</i>
JAOS	<i>Journal of the American Oriental Society</i>
JARCE	<i>Journal of the American Research Center in Egypt</i>
JBL	<i>Journal of Biblical Literature</i>
JCS	<i>Journal of Cuneiform Studies</i>
JEA	<i>Journal of Egyptian Archaeology</i>
JESHO	<i>Journal of the Economic and Social History of the Orient</i>
JNES	<i>Journal of Near Eastern Studies</i>
JNSL	<i>Journal of Northwest Semitic Languages</i>
JQR	<i>Jewish Quarterly Review</i>
JSOT	<i>Journal for the Study of the Old Testament</i>
JSOTSup	Supplements to <i>Journal for the Study of the Old Testament</i>
JSPSup	<i>Journal for the Study of the Pseudepigrapha: Supplement Series</i>
JSS	<i>Journal of Semitic Studies</i>
JTS	<i>Journal of Theological Studies</i>
KAI	H. Donner and W. Röllig, <i>Kanaanäische und aramäische</i>

Abbreviations

	<i>Inschriften, Mit einem Beitrag von O. Rössler</i> (vols 1–3; Wiesbaden: Harrassowitz, 1962–64)
KAT	Kommentar zum Alten Testament
KTU	Dietrich, Manfred, Oswald Loretz and Joaquín Sanmartín (eds) (1995) <i>The Cuneiform Alphabetic Texts from Ugarit, Ras Ibn Hani and Other Places</i> (KTU: Second, Enlarged Edition)
LB	Late Bronze Age
LC	Low Chronology (see §1.2.3.4)
LCL	Loeb Classical Library
LHBOTS	Library of Hebrew Bible/Old Testament Studies
LXX	Septuagint version of the Old Testament
MB	Middle Bronze Age
MCC	Modified Conventional Chronology (see §1.2.3.4)
MT	Masoretic text
NEA	<i>Near Eastern Archaeology</i>
NEAEHL	Ephraim Stern (ed.), <i>New Encyclopedia of Archaeological Excavations in the Holy Land</i> (4 vols; New York: Simon & Schuster; Jerusalem: Israel Exploration Society, 1992)
NEB	Neue Echter Bibel
OBO	Orbis Biblicus et Orientalis
OEANE	Meyers, Eric M. (ed.), <i>The Oxford Encyclopaedia of Archaeology in the Near East</i> (5 vols; Oxford)
OLA	<i>Orientalia Iovaniensia analecta</i>
Or	<i>Orientalia</i>
OT	Old Testament/Hebrew Bible
OTL	Old Testament Library
OTS	<i>Oudtestamentische Studiën</i>
PAPS	<i>Proceedings of the American Philosophical Society</i>
PdÄ	Probleme der Ägyptologie
PEQ	<i>Palestine Exploration Quarterly</i>
PJb	<i>Palästina-Jahrbuch</i>
RB	<i>Revue Biblique</i>
RBL	<i>Review of Biblical Literature</i>
RIMA	The Royal Inscriptions of Mesopotamia, Assyrian Periods
SAAS	State Archives of Assyria Studies
SAHL	Studies in the Archaeology and History of the Levant
SANE	Studies on the Ancient Near East
SBL	Society of Biblical Literature
SBLABS	Society of Biblical Literature Archaeology and Biblical Studies

Abbreviations

SBLASP	SBL Abstracts and Seminar Papers
SBLBMI	SBL Bible and its Modern Interpreters
SBLDS	SBL Dissertation Series
SBLMS	SBL Monograph Series
SBLSBL	SBL Studies in Biblical Literature
SBLSBS	SBL Sources for Biblical Study
SBLSCS	SBL Septuagint and Cognate Studies
SBLSymS	Society of Biblical Literature Symposium Series
SBLTT	SBL Texts and Translations
SBT	Studies in Biblical Theology
SEL	<i>Studi epigrafici e linguistici</i>
SemeiaSt	Semeia Studies
SHANE	Studies in the History of the Ancient Near East
SHCANE	Studies in the History and Culture of the Ancient Near East
<i>SJOT</i>	<i>Scandinavian Journal of the Old Testament</i>
SNTSMS	Society for New Testament Studies Monograph Series
SWBA	Social World of Biblical Antiquity
TA	<i>Tel Aviv</i>
TAD	Porten, Bezael, and Ada Yardeni (1986–99) <i>Textbook of Aramaic Documents from Ancient Egypt: 1–4</i> (Hebrew University, Department of the History of the Jewish People, Texts and Studies for Students; Jerusalem: Hebrew University)
TDOT	Botterweck, G.J. and Ringgren, H. (eds), (1974–) <i>Theological Dictionary of the Old Testament</i> (8 vols; Grand Rapids)
TRu	<i>Theologische Rundschau</i>
TSAJ	Texte und Studien zum Antiken Judentum
TSSI	J. C. L. Gibson, <i>Textbook of Syrian Semitic Inscriptions</i> (1973–81)
TWAT	Botterweck, G. J. and Ringgren, H. (eds), (1970–) <i>Theologisches Wörterbuch zum Alten Testament</i> (Stuttgart)
TZ	<i>Theologische Zeitschrift</i>
UF	<i>Ugarit-Forschung</i>
VT	<i>Vetus Testamentum</i>
VTSup	Supplements to <i>Vetus Testamentum</i>
WBC	Word Bible Commentary
WMANT	Wissenschaftliche Monographien zum Alten und Neuen Testament

Abbreviations

ZA	<i>Zeitschrift für Assyrologie</i>
ZAH	<i>Zeitschrift für Althebräistik</i>
ZAR	<i>Zeitschrift für altorientalische und biblische Rechtsgeschichte</i>
ZAW	<i>Zeitschrift für die alttestamentliche Wissenschaft</i>
ZDPV	<i>Zeitschrift des Deutschen Palästina-Vereins</i>
ZTK	<i>Zeitschrift für Theologie und Kirche</i>
§	Marks the sections of the text for cross-referencing

Preface

Throughout the writing of this book, its working title was, *Prolegomena to a History of Israel: What Do We Know and How Do We Know it?* In the back of my mind, though, I think I realized it would not be the final title. It was the publisher who objected on the grounds that it would not be readily understood by some of its intended readership. The subtitle expresses what I have been trying to do, and the 'Aims' (§1.1) explain in detail. It is with some regret that it has not been possible to use my original title, but perhaps to have done so would have been an act of hubris – at least, some of my colleagues might have thought so.

This book is, however, not a history of Israel but the preparation – the *prolegomena* – for such a history. It is aimed initially at scholars, with the intent of contributing to the current debate. By laying out as clearly as possible the main primary sources and drawing attention to the areas of debate and the arguments being advanced, I hope to give a snapshot of the field at the present time. Yet as always, I want to reach other audiences as well and have attempted to make some concessions for them, such as giving both the Hebrew script and transliteration. These secondary audiences include scholars who do not work primarily in history but who would like a useful overview and reference; scholars who may be historians but not specialists in Hebrew Bible or ancient Israel; students who might find that the book would serve as a useful text. In that vein, I have tried to make the data accessible and provide sufficient information even to non-specialists, even if this is sometimes by way of referring the reader to some relevant bibliography. Such a small book can cover only so much, but within the confines of the space allotted I hope to have given a helpful – and thoughtful – account about the problems and methods for writing a history.

Preface

There are many people whom I could thank for discussion, inspiration and other help provided, but this could be a very long list. In addition to those to whom the book is dedicated, I shall limit my thanks to the following: Jim Eisenbraun, for making available a copy of the Amihai Mazar Festschrift before formal publication; Benyamin Sass, for a copy of his book on the alphabet; my student Kwanghyun Ryu for bringing some bibliographical items to my attention.

My first class in the history of Israel was in 1970 with Professor Loren R. Fisher. Although my perspective at the time was not in line with his, I still managed to learn a great deal and to begin a journey on the road to learning a critical approach to the Bible.

In the decade since the European Seminar on Methodology in Israel's History (now shortened to European Seminar in Historical Methodology) had its first meeting, I have learned an enormous amount from my peers in the UK, on the Continent, from Israel and elsewhere. My main reason for founding the Seminar was to get scholars of different views and approaches to talk to each other. I was also hoping for a gradual narrowing of the range of opinions, which does not seem to have happened, but I think we have all become clearer about our own opinions and also those of others in the Seminar. In the process, I think some useful volumes have been produced for the wider scholarly world.

British universities have traditionally focused on undergraduates. For almost 25 years I have offered an optional module in the History of Israel on a regular basis. Because students in the class are new to the subject, most of my learning has come from repeatedly having to go over the material in preparation for the class rather than directly from students. But during that time I have had some interesting essays and found teaching the class very rewarding.

Kingston-upon-Hull, UK
13 October 2006

Part I

Introduction

CHAPTER 1

The Principles and Methods of Investigating the History of Ancient Israel

1.1 Aims

This book is not a history of ancient Israel, but it is the preparation for such a history. Its purpose is to survey the sources and summarize the data available for writing such a history, and then evaluate the various interpretations of these data for reconstructing a history of Israel. It is, in fact, 'prolegomena to a history of Israel'. It asks: what do we actually know? On what basis can we claim to have knowledge of ancient Israel? Simple questions, but the answers are far from simple. In order to address them, each chapter has three main parts:

1. Original sources. In this section the pertinent sources for a history of Palestine are surveyed and discussed, with relevant publications, translations and secondary studies.
2. Analysis. In this section specific topics will be examined, the data from the sources critically discussed and the various theories that have been advanced will be considered and evaluated.
3. Synthesis. Here the main issues will be summarized and an attempt made to outline how I would put all the data together and would understand the history of Israel for the period covered by the chapter.

The principles on which I shall work are summarized in §1.3.3 below, but first I want to survey some of the main questions that go into these principles and the main areas relevant for writing a history of ancient Israel today.

ANCIENT ISRAEL

1.2 Concepts and Complications: The Question of Sources and Methods

This section will take up several different issues that are relevant for writing a history. These might seem somewhat miscellaneous, but each (potentially) affects the reconstruction of ancient history and requires some knowledge or background on the part of the reader.

1.2.1 *The Place of the Social Sciences*

Although many biblical scholars make use of the social sciences, it is probably fair to say that Hebrew Bible/OT studies have still not fully incorporated the social sciences into their frame of reference. Not all studies relating to the Bible require use of the social sciences, but many studies that one sees do not go behind the biblical text, even though making use of insights from anthropology or sociology would give a better and more complete perspective. There is no question that anyone working on the history of ancient Israel needs to make full use of the contributions that can be brought in from the many social scientific studies.

The world of the social sciences is too large for a proper survey here, but for many points within the present study social scientific literature will be cited. I shall not normally make a hard and fast distinction between the terms ‘social anthropology’ (normal British usage) and ‘cultural anthropology’ (normal American usage), and professionals themselves do not always find it easy to distinguish anthropology from sociology. Archaeology has benefitted from the social sciences in recent years, and archaeology of all periods now routinely incorporates a great deal from anthropology. Some examples of areas where social theory and anthropology are invoked in this study are the settlement (§3.2.4.2), nomads and tribes (§3.2.4.3) and state formation (§3.2.4.4), but the social sciences leaven the work at many points and are essential for proper historical work in antiquity.

Unfortunately, biblical scholars have tended to be quite naive in their use of social anthropology, seeming to follow a stereotyped formula as follows: fish out a theory found in an elementary textbook or – preferably – one that is starting to make ground as a respectable topic. Take this theory, pick a set of unsuspecting biblical data, read the theory into the data, throw in some half-understood sociological vocabulary and – presto! – another sociological study. Therefore, some points to be kept in mind include the following (cf. Grabbe 2001c: 121):

1. Theories derived from the social sciences are simply models to be tested against the biblical and other data, not conclusions to be imposed on the sources. They are simply analogies based on one or more cultures. They are not 'facts' that can then be taken as givens by biblical scholars but are interpretations, to be critically examined against the data and then modified or discarded where necessary.
2. The function of sociological theories is to interrogate the textual or other data. The texts themselves may be read uncritically, as if they provided immediate access to the ancient society. It is easy to forget that the biblical texts cannot be treated like anthropological reports. A related danger is to overinterpret – to find a lot more data in a passage than is warranted because of applying the sociological theory. Cross-cultural comparisons allow us to ask questions, to look at familiar data in a new light, to think in new directions.
3. One of the most problematic tendencies in scholarship is that of reading modern ethical and theological concerns into the data. Biblical scholars are in the habit of making statements about the text based on theological bias and calling it sociological analysis. What should be sociological description becomes in fact an ideological value judgement. Too often sociological theory is simply a vehicle to import preconceived views about the biblical text. The city/urban is bad; the country/rural is good. The rich/ruling class is bad; the poor are good. The Canaanite city-states are bad; the refugee slave/peasant/proto-Israelite highland settler is good. The monarchy, social status, the economic situation, class divisions, urbanism and the like are all presented from the point of view of what a modern liberal, middle-class biblical scholar with a social conscience would consider acceptable.

1.2.2 *The Longue Durée*

One of the insights often forgotten in historical discussion is the extent to which any history is shaped by long-term factors that are usually outside the actors' control: physical geography (the geology, climate, vegetation, types of agricultural land, routes of communication and other physical surroundings), the availability of natural resources and the necessities of life (traditional lifestyles and modes of making a living) and long-term historical events (population movements, rise and fall of empires and nations, traditional alliances, old feuds and rivalries). It was the insight of the *Annales* school of historiography that different aspects of history have their own rhythm and temporal progress (for example, I. Finkelstein has drawn

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attention to the long-term cycling between settlement and pastoralism [§3.2.4.2]). Some historical constituents tend to change very slowly; others change much more quickly. This led F. Braudel (1980: 25–54) to divide history into three levels: (a) the level of events (*histoire événementielle*), the individual happenings on a short-term basis (though these do not necessarily escape the influence or sometimes even the determination of geography and environment); (b) the medium-term processes (*histoire conjonctures*); (c) the changes over long periods of time, affected by such issues as landscape, geology and climate (*longue durée*).

1.2.3 Using Archaeology as a Source

All historical work requires sources, by which is meant the source of data or information. Thus, ‘source’ includes not just literature or inscriptions but archaeology, surveys, demographic studies and so on. Any ancient history should depend as far as possible on ‘primary sources’, the principle already laid down by von Ranke (§1.3.1). This means that artifactual evidence – archaeology and material culture – is extremely important. In contrast to the old way of beginning with the text, historical reconstruction should start with the *longue durée* (geology, climate, environment), and then move on to inscriptions and archaeology. These furnish information not found in texts and also provide an objectivity not possessed by texts because they provide actual realia from the past. The importance of archaeology cannot, therefore, be overestimated.

1.2.3.1 General Comments

The use of archaeology for historical reconstruction is as complicated as the use of texts. The material data have to be interpreted just as much as textual data. Artifacts by themselves are mute. They speak only when interrogated and given a context by the archaeologist and the historian. Much depends on the skill and technical understanding of the excavators and the careful recording of the archaeological data. As David Ussishkin (forthcoming a) has put it:

The need for objectivity in archaeological research must be emphasized: Firstly, most data can usually be interpreted in more than one way: this or that wall can be assigned to the upper level but it may also be assigned to the lower. Or this and that storage jar can be assigned to a floor, but also to a pit dug from above.

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Or the uncovered fragment could be part of a bowl but it also resembles a jug. And so forth. Secondly, the archaeologist bears a great responsibility as in most cases he cannot repeat the discovery: uncovering the data is also the destruction of the data. For example, the archaeologist uncovers a jar lying in the debris – once he has extracted the jar we cannot repeat the discovery and study afresh the context of the same jar. Taking into account all the above I feel that the work of the archaeologist resembles that of a police detective, whose primary task when dealing with a case is to sort out the facts, and to interpret them in the most objective manner possible. Indeed, as I often state in lectures and in writing, I feel that my mentors are Sherlock Holmes and Hercule Poirot and their colleagues.

The past four decades have seen some significant changes in archaeological theory and method, some of which has had its impact on the archaeology of Palestine. This can only briefly be surveyed here. It is probably fair to say that the 'Biblical Archaeology' mode of operation had dominated the archaeology of Palestine up to the death of Albright who was its foremost practitioner (Dever 2001: 57–9). Perhaps it might be characterized in the phrase often quoted from Yigael Yadin, working with a spade in one hand and the Bible in the other. In other words, the Bible constituted the guide for interpreting the archaeology. 'Biblical Archaeology' was very much in the traditional cultural-historical archaeology mode, in which an emphasis was placed on describing, reconstructing history, explaining developments by cultural contact and diffusion and migration, and the identification of ethnicity with material culture – 'pots equal people' (cf. Renfrew and Bahn 2004: 40–2). It was partly the changes more broadly in archaeology that caused a serious questioning of both the historicity of the Bible (§1.3.2) and the assumptions of 'Biblical Archaeology' in the 1970s. W. G. Dever (1981; 1985) was one of the main scholars who drew attention to how the changes happening in the broader field of archaeology were eroding the old 'Biblical Archaeology'.

What is usually termed 'processual archaeology' – though often called the 'New Archaeology' – arose in the 1960s (Redman 1999: 60–5; Renfrew and Bahn 2004: 41–2, 473–84). It was an attempt to put archaeology on a more scientific basis, looking to anthropology and statistical and systems analysis. The general view was that society was a system, made up of a set of subsystems (subsistence, trade, technologies, etc.), and cultural change was to be explained by the processes and interactions of systems, especially as these adapted to their environment(s). Archaeology was an objective discipline, with the aim of explaining (not just describing) by logical argument and by the use of hypotheses and models that were, however, to be rigorously tested.

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In the late 1980s and early 1990s a reaction developed to what was seen as the excessively mechanistic approach of the New Archaeology: it was too determinative, ignored the place of the individual and did not recognize the subjective element in all archaeological interpretation. This was 'post-processual archaeology', or 'interpretive archaeology' as some preferred to call it (Redman 1999: 65–9; Shanks and Hodder 1995; Renfrew and Bahn 2004: 494–6). It had many parallels with the post-modernist movement in literature and other cultural fields taking place at the same time. Some of its main features are the following (Shanks and Hodder 1995: 5):

- It focuses on contextual concerns, including the natural environment but also the socio-cultural environment.
- All social activity (including doing archaeology) has to do with meaning and interpretation – making sense of things. Hence, the designation 'interpretative archaeology'.
- The person and work of the interpreter is explicitly recognized.
- Archaeology works in the present, constructing knowledge, narratives and the like from the remains of the past. It 'reads' material culture like a 'text'.
- There is no final or definitive account of the past; archaeology is a continuing process.
- Interpretations of society are less concerned with causes than making sense of things.
- Possible interpretations are multiple: different interpretations for the same set of data are possible.

This does not mean that the New Archaeology disappeared; on the contrary, its procedures still dominate the work of most archaeologists. But the insights of post-processual archaeology have also been recognized. It is probably the case that the dominant approach now is what has been called 'cognitive-processual archaeology', which seems to be the basic core of processual archaeology but well leavened by the interpretive approach (Renfrew and Bahn 2004: 496–501; cf. Redman 1999: 69–72). The main ways in which it differs from processual archaeology are given as the following (Renfrew and Bahn 2004: 496–7):

- It seeks to incorporate information about cognitive and symbolic aspects of society.
- It recognizes that ideology is an active force within society and on individuals.
- Material culture is an active factor in the process by which individuals

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- and society construct their world.
- Internal conflict within societies has to be taken into account.
- The creative role of the individual has to be recognized.
- Facts can no longer be considered independent of theory, nor are there universal 'laws of culture process' on the analogy of physical laws.

Most Palestinian archaeologists would probably pay strong lip-service to the 'cognitive-processual' approach, though it appears that most do not agonize a lot over theoretical method: they excavate as they have been taught and as their experience has informed their work over the years. But it is also the same with historians of my acquaintance – they just get on with it without long debates over the historical method. As Dever noted, however:

The result is that Syro-Palestinian archaeologists, despite some *rapprochement* with the field of anthropology in recent years, are still dismissed by our colleagues as parochial, naïve, and incapable of contributing anything to the advance of archaeology as a discipline. Again, some of our more Neanderthal colleagues ridicule any desire to be in the mainstream, but they are terribly wrong. (Dever 2003b: 515)

As is clearly evident, the debate still continues about the place of the Bible in archaeological interpretation. One can say that the 'biblical archaeology' of Albright and Yadin still has considerable influence on Palestinian archaeology. For some, at least, the Bible still seems to play a central role in the interpretation of Palestinian archaeology. Apart from that we need to consider two cautions relating to the use of archaeology in biblical interpretation.

First is the one that was being discussed above: failure to recognize that the use of archaeological data is just as subjective and interpretative as data from texts. Some archaeologists (and others) give the impression that it is objective, that archaeology somehow speaks by itself. Many times in the past we were assured that archaeology 'proved' this or that scenario. Some of that is still around, but most recognize the need for an interpretative context and the limitations of the science.

The second danger is the exact opposite: to assume that archaeological data is no more objective than textual data. Such an approach has mistakenly led some to discount its value:

... in the modern period of historiography it has sometimes been assumed that archaeological remains offer us the prospect of grounding historical statements in something more solid than testimony... This kind of view of the nature of

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archaeological evidence has been common among historians of Israel, even where they have sometimes recognized that it cannot be entirely correct and have found space in one part of their minds for the contrary idea ... that archaeological data are no more 'objective' or 'neutral' than other sorts... In fact, *all* archaeologists tell us stories about the past that are just as ideologically loaded as any other historical narrative and are certainly not simply a neutral recounting of the facts. (Provan, Long and Longman 2003: 63)

While this quote recognizes that artifacts indeed have to be interpreted, just as do texts, it ignores the major difference between the two sets of data: the archaeological data actually existed in real life – the artifacts are realia (apart from some faked artifacts – see below). Texts, on the other hand, are products of the imagination. The content of a text always contains human invention, and it is always possible that a text is entirely fantasy.

Archaeology is especially important because it has the potential of providing a separate witness to history and a source of distinct and independent data. The proper attention to archaeology is vital for any history of ancient Israel, and it is my intention to try to give it the prominence it deserves. But a number of studies in recent years have pointed out some long-term problems of interpretation in Palestinian archaeology. Not everyone will agree with some of the interpretations proposed, but the reader should be aware that some major reorientations are being called for by some archaeologists. Notice some of these:

1.2.3.2 Terminology of Archaeological Periods

A number of slightly different archaeological schemes, divisions and datings are used. One scheme of EB and MB (Albright 1932: 8–18; *NEAEHL* IV, 1529) divides EB I (ca. 3300–3000), EB II (ca. 3000–2700 BCE), EB III (2700–2200 BCE), MB I (2200–2000 BCE), MB IIA (2000–1750 BCE), MB IIB (1750–1550 BCE), or possibly three MB divisions: MB IIA–C. However, MB I is often labelled EB IV or Intermediate Bronze (Dever 1977: 82–9; Finkelstein 1995a: 87–8), in which case the MB (Dever 1987: 149–50; Ilan 1998: 297) is divided into MB I (ca. 2000–1800 BCE), MB II (ca. 1800–1650 BCE), and MB III (ca. 1650–1500 BCE), though some want to divide MB only into MB I and MB II. In spite of these differences, this concerns only the background history of the second millennium BCE.

For the history of ancient Israel, a major issue arises over the end of LB, beginning of Iron I, and end of Iron I (also, some divide Iron I into IA and IB). This, though, is not the main difficulty. The real problem is that at

the moment there is no agreed terminology among archaeologists for the period from about 1000 BCE to the fall of Jerusalem in 587/586 BCE. Some schemes are based on supposed 'historical' periods, which can be question-begging since most schemes tend to associate certain artifacts with particular historical periods. After the Hazor excavations of the 1950s (so Barkay 1992: 305), Aharoni and Amiran proposed Iron II (1000–840 BCE) and Iron III (840–586 BCE), but the *Encyclopedia of Archaeological Excavations in the Holy Land* (when first published in Hebrew in 1970 but also maintained in the English translation [Avi-Yonah and Stern (eds) 1975–78: IV, 1226]), had the following scheme: Iron IIA (1000–900), IIB (900–800), IIC (800–586). W. G. Dever notes that American archaeologists tend to begin Iron II about 920 BCE, whereas Israeli archaeologists usually begin it about 1000 BCE (Dever 1998b: 416). His own treatment ends Iron IIA at 722 and Iron IIB with 586 BCE. However, the editor of the volume to which Dever contributes (Levy 1998: x–xvi, esp. xvi) divides as follows: IIA (1000–900); IIB (900–700); IIC (700–586). But even among Israeli writers Iron II is differently divided. Barkay proposed Iron IIA (10th–9th centuries BCE), Iron IIB (8th), IIIa (7th and early 6th), IIIb 6th (586 to the late 6th BCE). The *NEAEHL* used the terminology Iron IIA (1000–900 BCE), IIB (900–700), IIC (700–586), Babylonian and Persian periods (586–332). However, A. Mazar (1993) divided slightly differently, with Iron IIA (1000–925 BCE), IIB (925–720), IIC (720–586). H. Weippert (1988) differed from them all, with Iron IIA (1000–900 BCE), IIB (925/900–850), IIC (850–587).

This only-partially controlled chaos means that readers must always pay attention to the scheme used by the individual author, and the individual author ought (though does not necessarily do so) to indicate to the reader the particular scheme being used. The present book is divided into chapters that cover particular periods of time, without being water-tight compartments. But because it draws on a great variety of archaeological writings, it will quote the data according to the scheme used by the particular writer. I have tried to give preference to the archaeological period (LB, Iron I, Iron IIA, etc.) rather than a date; however, if a specific date or even century is given, an indication of who is doing the dating is normally provided. This has been complicated by the LC, since material culture conventionally dated to the tenth century BCE would usually be dated to the ninth according to the LC.

1.2.3.3 Use of Survey Data

The archaeology of Palestine was transformed in the 1980s by the execution of large-scale surveys and the use of their results. The importance and value

of these surveys will become clear in the archaeological survey of each chapter. Yet the proper use of survey data also requires that one be aware of the weaknesses of surveys, in comparison with actual excavation results. Two examples of those drawing attention to problems are G. Lehmann (2003: 123–5) and O. Lipschits (2004: 101–3; 2005: 259–60 n. 249). Some surveys have been conducted much more rigorously than others. A lot depends on the thoroughness of the collection of surface pottery, but surface pottery is not always a good indicator of the actual periods of habitation. Similarly, the estimation of site size – which is very important for demographical calculations – can be quite difficult. Yet Lehmann, Lipschits and others go on to point out the value and importance of surveys, in spite of potential problems. The same applies to salvage excavations (Lipschits 2004: 101–3).

1.2.3.4 The Debate over the ‘Low Chronology’

One of the most controversial but also potentially important developments is Israel Finkelstein’s ‘Low Chronology’ (LC). This thesis, advanced in its first full form in the mid-1990s, dated most of the events and finds of the Iron I and Iron IIA about a century later than had been common among archaeologists. Finkelstein’s original study (Finkelstein 1996a) argued that conventional chronology is based on the twin pillars of the stratigraphy of Megiddo and the Philistine Bichrome ware. Since then the debate has widened considerably to take in radiocarbon dating, correlation with the Aegean, the context of the Assyrian expansion in the ninth century and other factors. A major opponent of the LC has been Amihai Mazar (2005) who has developed what he calls the ‘Modified Conventional Chronology’ (MCC). This is similar to the traditional chronology but extends the Iron IIA from 980 to 830, that is, covering most of both the tenth and the ninth centuries. This allows three major pottery periods (Iron IB, Iron IIA, Iron IIB), each lasting about 150 years, in the 450 years between approximately 1150 and 700 BCE.

The following are some of the main issues around which the arguments – pro and con – have revolved:

1. The biblical data. Finkelstein points out that conventional dating is strongly – if not always explicitly – influenced by the biblical text (e.g., 2005a: 34). This sometimes leads to circular arguments in which the argument depends on the biblical text and moves around to arguing that the data support the biblical text. A good example is Shoshenq’s

- invasion (see next point). Yet all parties have appealed to the biblical text in the discussion of the two key sites of Samaria and Jezreel (Ussishkin 2007; Finkelstein 2005a: 36–8).
2. The invasion of Shoshenq. This event has been a central benchmark for dating historical and archaeological data, yet the dating of when it happened has depended on the biblical text, since 1 Kgs 14.25–28 puts it in the fifth year of Rehoboam (Finkelstein 1996a: 180). A recent Egyptological evaluation has dated the event to 917 BCE (Shortland 2005). Yet this is not the end of the matter, because it is not clear exactly what sort of measures were taken by Shoshenq. It had been assumed that his invasion resulted in widespread destruction of sites, but it has been argued that if Shoshenq planned to use Megiddo as the place to plant his royal stela, he would hardly have destroyed the site (Ussishkin 2007). In fact, it ‘has not been proven that any sites were destroyed by Shishak in 925 B.C.E., and the attribution of destruction layers to the end of the tenth century at many sites is mere conjecture’ (Barkay 1992: 306–7).
 3. Implications for the Aegean and other Mediterranean areas. N. Coldstream (2003: 256) argues that the LC best fits the situation in the Aegean, but A. Mazar (2004) stands against this. Yet Killebrew (forthcoming) also relates the situation in Philistia to the broader Mediterranean pottery context and agrees that the LC ‘with some minor revisions’ would best fit the situation elsewhere in the Mediterranean: see next point.
 4. The development and dating of Philistine Monochrome and Bichrome pottery. It seems to be generally agreed that Philistine Monochrome (or Mycenaean IIIC:1b or Mycenaean IIIC Early to Middle) appears in the late twelfth century, developing from imported Mycenaean IIIC:1a (Mycenaean IIIC Early or Late Helladic IIIC Early). The Egyptian Twentieth Dynasty and the Egyptian presence in Palestine came to an end about 1140/1130 (Mazar forthcoming a). Yet no Philistine Monochrome ware appears together with Egyptian pottery of the Twentieth Dynasty, not even in nearby sites such as Tel Batash and Tel Mor, which suggests that Philistine Monochrome is post-1135 BCE, and the Philistine Bichrome is even later – the eleventh and perhaps much of the tenth century but not before 1100 BCE (Finkelstein 1995b: 218–20, 224; 2005a: 33). A. Mazar (forthcoming b) responds: (1) Canaanite pottery assembly in Ashdod XIII and Tel Migne is typical of the thirteenth and beginning of the twelfth centuries BCE, and Lachish VI must have been contemporary; (2) local Mycenaean IIIC is inspired by the Mycenaean IIIC pottery in Cyprus but this disappears after the

mid-twelfth century; (3) although D. Ussishkin and Finkelstein claim it is inconceivable that locally made Mycenaean IIIC did not reach contemporary sites in Philistia and the Shephelah, this ignores cultural factors that could limit it to a few urban centres; the early stage of Philistine settlement lasted perhaps only a generation, and Lachish is at least 25 km from the major Philistine cities which is sufficient to create a cultural border; (4) the Philistine settlement was possible perhaps because of the state of the Egyptian domination of Canaan at this time, and Philistine Bichrome pottery slowly emerged as a hybrid style later, probably during the last quarter of the twelfth century.

Yet in a thorough study of the Aegean-style pottery A. E. Killebrew (forthcoming a) argues that the high chronology (two-wave theory) would date the Philistine Monochrome (Mycenaean IIIC:1b) to about 1200 BCE as the result of an early proto-Philistine wave of Sea Peoples, while the middle chronology would date this pottery to about 1175 BCE, with the Bichrome developing from it in the mid-twelfth century. Such datings are becoming more problematic in light of the increasing consensus that Mycenaean IIIC:1b should be equated to Mycenaean IIIC Early (into Middle) which is dated to the mid-twelfth. With some revisions (such as dating the initial appearance of Mycenaean IIIC Early phase 2 to about 1160 BCE), the low chronology would best fit the dating of Mycenaean IIIC Early to Middle at other sites in the eastern Mediterranean and would also provide a more reasonable dating of Bichrome to the eleventh continuing into the tenth, based on LB II–Iron I stratigraphic sequences at both Tel Mique and Ashdod.

5. Pottery assemblages and the dating of various strata. Here there is a surprising difference of interpretation between professional archaeologists whom one would expect to agree about the facts in the ground: the relationship between the strata in the sites of Hazor, Samaria, Jezreel and Megiddo; the interpretation of Jerusalem; the dating of Lachish; the dating of the Negev destruction.
6. The gap in the ninth century. Current dating leaves a strange gap in the ninth century in the dating of archaeological remains (Finkelstein 1996a). We have much in the tenth and the eighth centuries but not the ninth, leaving a very thin stratigraphy over 350 years. For example, Tel Mique and Tel Batash have thick accumulations related to Philistine Bichrome ware, a ninth-century gap, then limited Iron II remains. Other sites show a similar gap: Tell Halif, Tel Mor, Tell Beit Mirsim, Ashdod, Tel Haror, Gezer, Jerusalem. The LC closes the unexplained gap between monumental architectures of the tenth century and evidence of public administration for the late ninth to the eighth centuries BCE.

7. Downgrading or elimination of the united monarchy. The monuments previously associated with the united monarchy are redated from the second half of the tenth century to the early ninth. It strips the united monarchy of its monumental buildings, including ashlar masonry and proto-Ionic columns. We have evidence of fortifications in the tenth century, but the main mounds in the north (Megiddo and Gezer) and the south (Beersheba and Lachish) only date to the ninth century or later. This means that the strong and historically attested Omri kingdom is the first state in Palestine and preceded the geographically weaker Judah. Taking a global perspective, this is what one would expect rather than the anomalous Jerusalem-centred and Judah-dominated kingdom of David and Solomon.
8. Radiocarbon dates. This is the most recent attempt to find a way to pin down the matter of dating and has great potential. Yet a database of radiometric dates from a wide variety of sites is needed; such is now being developed and may resolve the issue (Sharon *et al.* 2005; Sharon *et al.* forthcoming). In the meantime, there are significant differences in interpretation. The reasons for this have been well laid out by A. Mazar:

The many stages of selecting the samples, the pre-treatment, the method and process of dating, and the wide standard deviation of Accelerator Mass Spectrometry dates may create a consistent bias, outliers or an incoherent series of dates. The calibration process adds further problems, related to the nature of the calibration curve in each period. In our case, there are two difficulties: one is the many wiggles and the shape of the curve for the 12th-11th centuries BCE. This leads to a wide variety of possible calibrated dates within the 12-11th centuries BCE. The other problem is the plateau between 880 and 830 BCE, and the curve relating to the last third of the 10th century BCE, which in certain parts is at the same height as the 9th century BCE plateau. In many cases the calibrated dates of a single radiocarbon date is in both the 10th and the 9th centuries BCE, and in the 9th century BCE more precise dates between 880 and 830 BCE are impossible. These limitations are frustrating, and make close dating during this time frame a difficult task. It seems that in a debate like ours, over a time-span of about 80 years, we push the radiometric method to the edges of its capability, and perhaps even beyond that limit. (Mazar 2005: 22)

The result is that a variety of radiocarbon dates have been made at key sites, with some arguing that they support the MCC (Mazar 2005: 22-3; Mazar *et al.* 2005); and others, that they support the

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LC (Finkelstein and Piasezky 2003; Piasezky and Finkelstein 2005; Finkelstein 2005b; Sharon *et al.* 2005; Sharon *et al.* forthcoming).

1.2.3.5 The Stratigraphy of Samaria

For many years Samaria was used as a model for interpreting other sites. The ancient site was excavated by the Harvard University expedition in 1908–10 and then under a combined expedition under J. W. Crowfoot in 1931–35; a few isolated digs have taken place since then. As part of the Crowfoot expedition, Kathleen Kenyon presented the stratigraphy of the site (1942). R. Tappy has compared the published interpretation from that in Kenyon's notes and found many discrepancies (1992; 2001; §4.1.1).

1.2.3.6 Reinterpretation of the Yochan Seal

The following bulla, once dated to the time of Jeremiah, should now be placed in the late eighth century: 'Belonging to Eliaqim servant of Yochan' (לְאֵלִיָּקִים נֶעֶר יֹכָן): Gogel 1998: 467 [Beth Shemesh seal 2], 492 [Ramat Rahel seal 8], 494 [Tell Beit Mirsim seal 1]; *AHI*: 100.108; 100.277; 100.486; Garfinkel 1990). Impressions of this seal on jar handles were found at Tell Beit Mirsim and Beth Shemesh, and eventually at Ramat Rahel. When impressions of this seal were first found, the name Yochan or Yochin was thought to be a version of King Jehoiachin's name. Based on the interpretation of W. F. Albright scholars long took this as the seal of Jehoiachin's steward, which resulted in the misdating of the strata where they were originally found and those strata elsewhere thought to be parallel. In Garfinkel's words this caused 'sixty years of confusion' in Palestinian archaeology. Although this interpretation is still widespread, especially in more general works, it has now been completely reassessed by specialists (Ussishkin 1976; 1977; Garfinkel 1990). Even if some aspects of the seals and their owners have still not been clarified, they have now been dated to the period before 701 BCE.

1.2.3.7 Problems with Forgeries

Becoming much more prominent in recent years is another danger: the faking of antiquities being traded on the market (Rollston 2003). This has been especially a problem with seal impressions, but recently inscribed objects

have appeared. A number of these have been exposed with more or less a scholarly consensus (e.g., the Jehoash inscription), but there are others still in dispute. The good side of this is that objects found in properly controlled excavations are accepted as authentic. A genuine archaeological context is usually sufficient guarantee that the finds are genuine. Very seldom has the authenticity of such objects been questioned (though some have queried the Tel Dan inscription [§4.1.3.1]).

As more and more items are obtained through the antiquities market, the bigger the potential problem becomes. We now find that a number of discoveries hailed as evidence of the biblical account are questionable or even out-and-out fakes. To take one cherished example, even the famous Berekyahu ben Neriya seal impressions, alleged to belong to the scribe of Jeremiah, now seem suspect. The first one found was published by Avigad (1978). More recently another has allegedly been discovered and was published by Deutsch and Heltzer (1994: 37–8) and Shanks (1996). This latter seal impression is part of the indictment against Robert Deutsch and Raphael Brown (Rollston and Vaughn 2005: 4). It is hardly unusual that Avigad (1978; 1986) put particular emphasis on the Baruch seal impression when he set about publishing the lot of seals of which they were a part, the so-called ‘Burnt Archive’. Avigad also asserted that there ‘was no reason to suspect their authenticity, and I seriously doubt whether it would be possible to forge such burnt and damaged bullae’ (1986: 13). I have not seen this first impression questioned in print (though I keep hearing rumours), but now that the second one is alleged to be forged, the first cannot fail to be queried. It is always possible that the second one is a counterfeit copy of the first, but when the first is among a lot of unprovenanced seal impressions bought on the antiquities market, the question of authenticity must be forcefully raised for it as well.

We may be rapidly coming to the point of having to take a ‘Popperian’ view of the situation. By this, I mean that antiquities not obtained within a proper archaeological context can never be authenticated; they can only be falsified. That is, just as a scientific theory – according to Karl Popper (1959) – can never be proved but only falsified, so antiquities-market artifacts that have not been falsified will still retain a certain aura of uncertainty. There is an unfortunate air of naive arrogance on the part of some technical scholars. C. Rollston (2003: 183–6) tells of a laboratory which managed to create a fake patina that would pass most tests for authenticity, yet the very people able to do this took the stance that forgers would not be able to do the same. Many intelligent people through the ages have underestimated the ability of frauds and cheats to fool them, to their cost. When potentially large sums of money are at stake, it is worth the while of a good forger to invest the

necessary time, money and equipment to produce a plausible counterfeit. And those who think that antiquities cannot be so cleverly forged that they cannot fool modern laboratory tests or the judgement of expert epigraphers ought to read some of the recent discussions of the subject (for a sample, see Cook 2005; Goren 2005; Goren *et al.* 2004; Lemaire 2005; Rollston 2003; 2004; 2005; Rollston and Vaughn 2005; Vaughn and Dobler 2005; Vaughn and Rollston 2005).

It is not surprising that some epigraphers do not accept the judgement of the Israel Antiquity Authority's committees on various objects, and it is quite fair for them to present their arguments (e.g., Lemaire 2005). But headlines such as 'Forgery Hysteria Grips Israel: Is Everything a Fake?' can leave non-specialists with a rather distorted picture of what is consensus and what is genuine controversy. Museums around the world are filled with artifacts relating to ancient Israel and Judah that no one questions. It is the unprovenanced objects sold on the antiquities market that cause the problems.

1.2.4 *Ethnicity*

The question of ethnicity has been much discussed in recent decades, not only in biblical studies but also in social anthropology. Particular problems are those of group identity and identifying ethnic groups from material culture (i.e., can this be done from archaeology alone?) There is a problem already at the beginning of the discussion: what does one mean by 'ethnic' or 'ethnicity'? At the most basic level, it has to do with the Greek word *ethnos* which is variously translated as 'people, nation, race'. But this only partially answers our question. The basic issue is that throughout the historical sources are the names of groups and peoples, from the frequent naming of 'Israel' and 'Judah' and continuing down to obscure groups mentioned only once or twice in extant sources. How do we characterize these groups, who are often little more than names? Do we think of them in social terms, kinship terms (lineal? segmental? tribal?), ethnic terms or what? Was the name in the source their name for themselves or something alien that they never used? How we treat these groups in a historical discussion needs to take into account recent discussions of 'ethnicity' and '(ethnic) identity'. In most cases, we have no information beyond the textual data, though sometimes there is also material culture – or at least some scholars have brought material culture into the discussion of ethnicity/identity.

There have been different approaches to ethnicity not only in biblical scholarship (Brett 1996; Sparks 1998; Killebrew 2005: 8–16) but extensively

in anthropological study (Shennan [ed.] 1989; Hutchinson and Smith [eds] 1996; Sokolovskii and Tishkov 1996; Keyes 1997). A view that the ethnicity should be seen mainly in biological terms (ethnic groups have a common ancestry or kinship or genetic pool) is widely rejected but has contributed the important insight that ethnic groups generally define themselves in kinship or quasi-kinship terms. Others saw the question in terms of distinct cultures, but this was problematic in that cultural groups do not always develop an ethnic identity or group consciousness. There is also the fact that there is a 'primordial' quality to ethnic identity in which the group's distinctiveness – 'we/they' – is essential (Geertz 1973: 255–310; Keyes 1997). The classic study is that of F. Barth (1969) who pointed to the importance of inter-group boundary mechanisms: ethnic groups define themselves in contrast with other groups ('self-ascription'), often by a minimum of explicit (even trivial) differences. He explicitly rejected the use of an inventory of cultural traits (as Kletter [2006: 579] points out, W. Dever [1993: 22*–24*; 1995b: 201; 2003a: 192–3] seems to have misunderstood Barth in this regard and adopted the very definition that Barth criticizes). But there has been a good deal of discussion since Barth (Kamp and Yoffee 1980; Shennan [ed.] 1989; Hutchinson and Smith [eds] 1996; Finkelstein 1997; Jones 1997; Bloch-Smith 2003; Kletter 2006).

Because our knowledge of groups in the ancient Near East is based on texts rather than a direct study of living peoples, we are limited by what the texts tell us. This means that the task of penetrating to identity and ethnicity is often very complicated. For example, the biblical text tells us what was distinctive about Israelites and Judahites that made them different from the surrounding peoples, but what we have are the views of the biblical writers, not necessarily of the average Israelite or Judahite who might have seen his or her identity in quite different terms. For many groups (e.g., the Canaanites) we have only the views of their opponents and do not know how they regarded themselves. An interesting example is the situation in the Persian period, with the text of Ezra making a sharp distinction between 'Israel' and the 'peoples of the land(s)', but there is reason to think that both groups were Judahites and did not necessarily regard themselves as separate in the way that the textual writer portrays it (Grabbe 2004a: 285–8).

Trying to find a definition of an ethnic group is still not easy. Recent treatments tend to recognize the fluidity of ethnic identity (an insight from Barth), and any definition must recognize that. Kamp and Yoffee stated that most sociologists and anthropologists saw an ethnic group as 'a number of individuals who see themselves as being alike by virtue of a common ancestry, real or fictitious, and who are so regarded by others' (1980: 88). Kletter follows A. D. Smith in seeing an ethnic group as:

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... a group of people who share most – but not necessarily all – of the following: (1) a collective proper name; (2) a myth of common ancestry; (3) historical memories; (4) one or more differentiating elements of common culture; (5) an association with a specific homeland (which may be symbolic, without physical control of the homeland); and (6) a sense of solidarity among at least parts of the group. (Kletter 2006: 574)

Sokolovskii and Tishkov give a similar definition and suggest that it ‘opens further avenues for integration of anthropological, political and psychological knowledge in understanding of ethnic phenomena’ (1996: 192). Of particular interest is that self-identity may be strongly based on religion, myth and law, areas which have traditionally been studied with regard to ancient Israel.

Yet even such carefully thought out definitions can be in danger of restricting the recognition of how complex the matter is in the real world. Politicians may mount a self-serving campaign to encourage their constituents to think of themselves as of a particular ethnic group. Individuals might adopt a particular ethnic identity for the sake of social or economic advantage or even as a strategy for survival. Ethnic groups can grow out of what were once different ethnic groups (as will be argued below in reference to Israel [§3.2.4; §3.3]). The fact that a name occurs in a text does not necessarily tell us whether it represents an ethnic group. As will be discussed below (§2.2.1.5), the term ‘Canaanite’ probably applies to territory rather than ethnicity; that is, a ‘Canaanite’ seems to be the inhabitant of a region or land rather than the member of an ethnic group.

Finally, we come to an issue being very much debated at the present: the question of whether ethnic identity can be determined from material culture – also known as the ‘pots equal people’ debate. Relating material culture to ethnicity is fraught with problems (Edelman 1996b; Skjeggessald 1992). It is interesting that most of Kletter’s six elements relating to ethnic identity leave no trace in the archaeology record. Also, differences or similarity in material culture do not necessarily show differences or identity in ethnicity: they may be the result of similar or diverse environments (Finkelstein 1998b: 359, 365) or similar or diverse lifestyles. Nevertheless, attempts continue to be made to find ways through the obstacles and develop approaches that can successfully relate ethnicity and archaeology (cf. Redmount 1995; Killebrew 2005; Miller 2004; Kamp and Yoffee 1980; Jones 1997; Finkelstein 1997; Dever 1993; 1995b; 2003a: 191–200). Yet any list of particular traits in the material culture is likely to be problematic, whether we think of pottery, architecture, diet, or even language. The traditional list of ‘Israelite ethnic markers’ have evaporated as exclusive indicators: four-room pillared house,

collar-rim jar, abstinence from pork (Bloch-Smith 2003: 406–11; Kamp and Yoffee 1980; Kletter 2006: 579; Hesse and Wapnish 1997). What is clear is that there ‘is no list of material traits that can cause “ancient Israel to stand up” independently, without the help of written sources’ (Kletter 2006: 579).

It is interesting that neither Dever nor Killebrew identify Israel and her neighbours from material culture alone. On the other hand, we do have some written sources for ancient Israel, and why should we not use them? Particular traits might not prove an ethnic identity, but the clustering of certain traits might be diagnostic if combined with particular types of textual data. From Killebrew’s data it strikes me that the archaeology most clearly shows the presence of Egyptians (administrators, soldiers, etc.) and the Philistine settlement. Yet Bunimowitz (1990) argues that the actual material culture at the Philistine sites is a mixed one, and some ‘Philistine’ pottery is found in what are usually regarded as non-Philistine areas. Most importantly, for disputed sites the material culture has not been able to decide the question of who lived there (Finkelstein 1997: 220–1). We can end this preliminary discussion by asking three questions:

- Was ‘Israel’ an ethnic group? The answer is that both the people themselves and outsiders saw a group identity that we would call ethnic. The fact that a diversity of groups may have contributed to their makeup (cf. 3.2.4.4) – indeed, most probably did – does not negate ethnic identity (*pace* Lemche 1998b: 20–1).
- Were ‘Judah/Judahites’ to be identified as ‘Israel/Israelites’? Within the biblical text they seem to be seen as one ethnic group, yet they are never identified as such by outsiders. Judah/Judahites are never called Israel/Israelites in any non-Jewish literature or documents until the beginning of the Common Era.
- How do ‘tribes’ relate to the question? This issue is discussed below (§3.2.4.3).

1.2.5 Ideology and Neo-Fundamentalism

A fact all historians and scholars live with is the subjective nature of much argument (Grabbe 2002). It has been explicitly recognized in post-modernism (§1.3.1), but it was known long before that. Complete objectivity is an unobtainable ideal (many reject it even as an ideal), but most historians think that an effort should be made to be dispassionate in evaluating and arguing a case (§1.3.1). Yet S. Sandmel (1979) drew attention to a tendency

we all live with: to rely on the ‘comfortable theory’. What he pointed out was the extent to which we as scholars – and despite our implied adherence to critical thought – tend to gravitate toward those theories or views that we find most congenial: ‘A comfortable theory is one which satisfies the needs of the interpreter, whether theological or only personal, when the evidence can seem to point in one of two opposite directions’ (Sandmel 1979: 139). To give my own interpretation of Sandmel’s thesis, the comfortable theory is not a disinterested search for the ‘facts’, the ‘truth’ or whatever term one wishes to use, but a means of maintaining our position with the least amount of effort. When Sandmel exposed the ‘comfortable theory’, he did not exempt himself from his own strictures, nor can I. Sandmel admitted that he was as susceptible to the seductive comfortable theory as the next man, and I have to own up to the same temptations. We all have our own comfortable theories. Perhaps we would like to think that we have thought them through carefully and are willing to change our minds if the facts show otherwise, but whether in practice that is true is not something we individually can judge alone – our peers might have definite views on the matter!

We also all have ideologies that make us gravitate toward certain ‘comfortable theories’. Some use this, however, to argue that one theory is as good as another (some post-modernists) or that a fundamentalist interpretation of the Bible is as valid as a critical one. The post-modernist question is dealt with elsewhere (§1.3.1), but here we need to confront directly and explicitly the neo-fundamentalist approach to scholarship that is very much in evidence in some areas (Grabbe 1987; 2002). The term ‘maximal conservatism’ was coined by J. Barr (1977: 85–9). He notes that instead of advancing a dogmatic approach, conservative evangelicals will often adopt a position that can be defended from a critical point of view but is the one that is closest to the biblical picture. A ‘maximal conservative’ interpretation may be adopted by someone who is not a true fundamentalist. The old fundamentalism was generally easy to identify because it was blatantly and unequivocally apologetic about the Bible. Indeed, it was not unusual for the tone to be anti-intellectual. Neo-fundamentalism is just as fundamentalist, but it tends to cloak its defence of the Bible in the rhetoric of scholarship.

A truly fundamentalist approach will usually show itself by the assumption that the Bible is inerrant in the autographs, whereas a ‘maximal conservative’ approach will usually have moved away from such a stance. (Barr does not seem to make a distinction between ‘conservative evangelical’ and ‘fundamentalist’, but in my own experience there is a distance between those conservatively inclined and the true fundamentalist who believes in ‘the

inerrancy of the Scriptures’.) However, in practice it is not always easy to tell the difference between the two. The neo-fundamentalist will often adopt a maximal conservative approach without admitting the true guiding principle of his or her work in biblical studies. On the other hand, a fundamentalist approach does not have to be confined to the Bible-believers. A dogmatic scepticism that continually looks for a way to reject or denigrate the biblical text can be as ‘fundamentalist’ in its own way as a refusal to allow the Bible to be wrong. This attitude was well addressed by Hans Barstad in his essay on ‘bibliophobia’ (Barstad 1998).

There is no permanent state of purity nor any established chair of righteousness in scholarship. Even if one suspects that a scholarly position or theory is ideologically motivated – whether from biblical fundamentalism or some other ideology – one should evaluate the position on the basis of stated arguments. Trying to second guess motives has become too much of a pastime in the academy. This brings us to a related issue: the *ad hominem* argument.

1.2.6 ‘Maximalists’, ‘Minimalists’ and the ad hominem Argument

As noted below (§1.3.2), the history debate took on an unwelcome shrillness in the 1990s. The tendency for *ad hominem* argument and personal attacks was especially evident in the debate over ‘minimalism’ and ‘maximalism’. These terms have come into common usage in recent years to characterize the approach of scholars to reconstructing the history of Israel. These have been defined as follows: ‘minimalist’: one who accepts the biblical text only when it can be confirmed from other sources; ‘maximalist’: one who accepts the biblical text unless it can be proved wrong (Knauf 1991c: 171, though W. Hallo claims to have coined the terms [2005: 50; 1980: 3–5, nn. 4, 11, 12, 23, 55]). This designation has a certain usefulness in that it captures the dichotomy in how different scholars approach history and thus helps to explain and characterize some radical differences of interpretation of the same data. Yet in the end it is mainly a hindrance to discussion, for several reasons:

- Almost all scholars are minimalists in certain parts of history. That is, most scholars have now abandoned the picture of the biblical text for the ‘patriarchal period’ and the settlement in the land. Even though many still accept the concept of a united monarchy, few would follow the picture of David and Solomon’s kingdom as found in Samuel–Kings or Chronicles.

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- There are very few true maximalists, that is, those who accept the testimony of the Bible unless it be falsified. Indeed, the only history known to me so far written that one could call 'maximalist' is that of Provan, Long and Longman (2003; reviewed in Grabbe 2004c; forthcoming).
- Some of those pursuing a minimalist interpretation sometimes give the impression that they alone are exercising methodological rigour and that they are eschewing the subjective interpretations of others. This can be misleading, since all historical work is done within a theoretical framework. Apart from total scepticism, all attempts to understand the past make certain assumptions and depend on certain data. Thus, reconstructing the history of Palestine based purely on archaeology might seem more methodologically 'pure' than using texts, but this fails to realize that the context for the archaeological interpretation has been created by considerable use of texts (Miller 1991; Ahlström 1991; Dever [2001: 88; 2005: 77] seems to have misunderstood this point). Some who adopt the label 'minimalist' have drawn enthusiastically on reconstructions based on texts when it suits their purpose (for an example, see Grabbe 2000d).

It has become common in recent years to introduce personal motives into arguments: 'so-and-so takes this position because he/she is/thinks/believes this or that'. Unfortunately, we can always find reasons to say that someone takes a scholarly position because of personal or ideological motives (see the well-founded plea of Lemche 2000). Such statements have no place in scholarly argument. In fact, there is probably not a one of us who has not taken a position on some issue for personal reasons, even if totally unconscious of this motive. As suggested above (§1.2.5) I am very sensitive to arguments or positions that seem to arise from a fundamentalist stance with regard to the Bible. Yet, as John Emerton once remarked from the floor in a conference, we should reply to the specific arguments rather than what we think might lie behind them (cf. Grabbe 1987).

To take just one example, two British scholars published an article putting the case for dating the Siloam inscription to the Maccabaeon period (§5.1.1). This was attacked in an article by a number of scholars which had the heading of 'pseudo-scholarship'. Some of those who published in the article made some relevant and serious points, but the main abuse of editorial privilege was the header of 'pseudo-scholarship'. No doubt it was supplied by the editorial staff, not the contributors, but it served to prejudice the readership from the start. The redating of the Siloam inscription may be wrong – and most so far think it is – but it is

not 'pseudo-scholarship', and such *ad hominem* comments do not belong in scholarly writing or debate.

1.3 The Contemporary Practice of Writing Israel's History

It would probably not be an exaggeration to say that there is something of a 'crisis' – or at least a major impasse between two basic positions – with regard to the history of Israel. It is unfortunate but true that many of those taking a position are not really historians and do not seem to understand the issues well enough to qualify for entrance into the debate. But the existing fact – which underpins my reasons for writing this book – is that at present there is a major debate about writing the history of 'ancient Israel' (this term is used for convenience; some would prefer terms such as 'ancient Palestine' or 'southern Syria' or 'the Levant'). This has been widely – but inaccurately – characterized as a debate between 'minimalists' and 'maximalists'. Such a characterization is a caricature since it fails to note the wide variety of positions or principles of working among different scholars (on 'minimalists' and 'maximalists' see the discussion at §1.2.6).

In order to understand this debate – taken up in detail in the subsequent chapters of this book – it is useful to indicate how things have developed and where we stand generally in relation to historical study. I shall, first, give a survey of the debate about writing history in general, and then indicate how the writing, specifically of Israelite history, has developed in the past 40 years or so.

1.3.1 *Developments in General Historiography*

In order to discuss the history of ancient Israel, it is useful to survey how the science of historiography has evolved in recent times (for a more detailed survey and bibliography, see Grabbe 2004a: 2–10). It is fair to say that the principles developed by Thucydides almost 1500 years ago in many ways still guide the historian (cf. Grabbe 2001b), yet much has also been learned in the past couple of centuries.

In the nineteenth century important developments toward the modern study of history were made by Leopold von Ranke, but he is often misunderstood: he belonged to the Romantic tradition and believed in the importance of intuition in historical reconstruction, as well as careful research based on documentation. Ranke's contributions can be summarized under three headings (Evans 1997: 16–18): (a) establishing history as a

discipline separate from both philosophy and literature, (b) seeing the past in its own terms rather than judging by the standards of the present and (c) a critical method of source criticism based on philological principles. He made the important distinction between primary and secondary sources (Iggers 1997: 24). In his view the goal was to write history 'as it really [or "essentially"] happened' ('wie es eigentlich gewesen ist'). This famous quote, important as it is as a symbol, is often misapprehended. The context for Ranke's statement was that, contrary to previous historians who saw it as their right to pronounce judgement on history, he felt the responsibility of the historian was only to write what had happened, not moralize about it. Perhaps the most far-reaching impact came from the new discipline of the social sciences, toward the end of the nineteenth century. There was a shift in emphasis to social and economic trends rather than the actions of individuals in the political sphere. This application of the social sciences to historical study had its major impact after World War II. Especially important was the 'Annales School', of whom one of its leading proponents was F. Braudel; they emphasized the significance of the *longue durée* perspective (§1.2.2).

A watershed was marked in the 1960s with what is often called the 'linguistic turn' in historical study. There were several trends. Some of these were important developments but did not cause a major break with the past, such as the desire to write history 'from below' – to focus on the effect events had on individuals, the common people; to write the story of the ordinary soldier or the ordinary citizen; to recognize the common omission of women and other minorities from conventional histories. The 'grand narrative' so characteristic of some world histories of the nineteenth century and earlier had by and large already been rejected, but the tendency for many historians was now to work on what some called 'micro-histories'. The optimistic view of continual progress gave way to a more sceptical view of such a perspective. Finally, there was the debate on epistemology that had been underway for a long time in philosophy and elsewhere, owing much to Friedrich Nietzsche and Jacob Burckhardt.

At this time a radical questioning undermined the two major assumptions that had undergirded historical work from Thucydides to Braudel: the correspondence theory of history and the view that human intentions were the basis of human actions. This fundamental questioning came from what has broadly been called 'postmodernism', though in fact a great many different considerations – philosophical, literary, linguistic, anthropological, feminist – fed into the debate on historical methodology. The view now championed by some was that objective historical inquiry was not possible. How to define or characterize 'postmodernism' as it applies to history is not an easy task since postmodernists themselves often seem to avoid a positive

statement of their historical method. One useful way of summarizing the movement, however, may be the following (Appleby, Hunt and Jacob 1994: 198–237; Iggers 1997: 6–16, 118–33; Zagorin 1999):

- There is no essential difference between history and literature, between a historical narrative and a narrative of fiction. In the words of Hayden White, ‘historical narratives are verbal fictions, the contents of which are as much *invented* as *found* and the forms of which have more in common with their counterparts in literature than they have with those in the sciences’ (1978: 82, italics in the original).
- The world of the text is self-contained; nothing exists outside the text, because no reality can transcend the discourse that expresses it.
- There is no possibility of certain knowledge; there is no truth, only ideology. ‘The basic idea of postmodern theory of historiography is the denial that historical writing refers to an actual historical past’ (Iggers 1997: 118), that there is an objective truth separate from the subjective thought of the historian. Or one could point to the statement of Foucault that ‘reality does not exist, that only language exists’ (Iggers 1997: 133).
- The text can be interpreted in multiple ways. Authorial intent is an illusion. Texts conceal as much as they reveal, which means they need to be deconstructed to bring out the hidden assumptions, contradictions and gaps; furthermore, meaning is always deferred.
- The ‘grand narrative’ is at an end, to be replaced by fragmentation, with many different even competing histories and the inclusion of groups formerly omitted or marginalized. The key phrase is ‘history from below’ but also a shift to a focus on culture.

The radical antihistoristic elements of postmodernism are not in fact completely unexpected, as a close scrutiny of the developments in historical theory in the past two centuries shows (cf. Grabbe 2004a: 2–6). Nevertheless, the debate about postmodernism continues in a vigorous fashion among professional historians, at least in some parts of the Academy in English-speaking scholarship. One of the main advocates for a postmodern perspective in history, Keith Jenkins, has recently produced a ‘postmodernist reader’ (1997) that tries to bring together some of the most influential articles in the debate. In a long introduction he lays out the main issues, with a defence of his own approach. One can find a similar advocacy of postmodernism, though perhaps less flamboyantly presented, by Alun Munslow (1997), but the past few years have been especially characterized by a strong resistance movement. Joyce Appleby, Lynn Hunt and Margaret

Jacob (1994) produced a book which might appear at first to have a postmodern agenda in its assault on the way 'outsiders' (women and other minorities) have been excluded or neglected. Yet a good part of their text is a strong attack on such postmodern gurus as Foucault and Derrida.

A book achieving widespread circulation in the UK is Evans's *In Defence of History* (1997), which is written for a non-technical readership. It actually tries to explain clearly the different approaches of recent writers on historiography and is not just an assault on the postmodern, but it ultimately rejects postmodernism from being the way forward even if the relevance of some aspects of it is accepted. A wide-ranging attack on a number of recent trends in history-writing, as the title *The Killing of History* already makes quite plain, has been carried out by Keith Windschuttle (1996). He covers more than postmodernism, and such figures as Derrida are mentioned mainly in passing; however, he has a long chapter attacking Foucault whom he sees as the main culprit in undermining the traditional study of history. Although the title and style might suggest an irresponsible blunderbuss attack on ill-defined targets, the book makes a number of effective points despite some shortcomings (Gordon 1999). C. B. McCullagh (1998) has tried to steer a middle way by recognizing that the critics have not always presented the arguments of the postmodernists fairly and by himself giving due weight to the postmodernist positions; nevertheless, taking into consideration the valid points about subjectiveness and the place of language in reality, he still concludes that historical knowledge is possible (cf. also McCullagh 1991).

In the end, however, one can only agree with the observation that 'the majority of professional historians ... as usual, appear to ignore theoretical issues and would prefer to be left undisturbed to get on with their work while no doubt hoping the postmodernist challenge will eventually go away' (Zagorin 1999: 2). This certainly fits the attitudes of most historians I know in my own university who seem to have little interest in the debates on theory. Perez Zagorin has recently divided the reactions to the 'linguistic turn' into three sorts (1999: 3). Whether another division might be truer to the real situation is for others to say, but Zagorin's three-fold categorization is certainly clearer and more understandable than the rather confusing attempt to find five different positions by Jenkins (1997: 21-4):

- 'Some who evince great alarm at the incursions of postmodernism, considering the latter as a new kind of nihilism threatening the very existence of history as an intellectual discipline, and who tend to regard themselves as a beleaguered minority defending the citadel of reason against its hordes of enemies' (e.g., K. Windschuttle).

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- A small number of those who have embraced postmodernism (e.g., K. Jenkins).
- Those who feel that historians still have something to learn from the challenges and questions raised by postmodernism but have rejected it as a framework or at least its more extreme conclusions (e.g., R. J. Evans).

Among historians there is at present no uniform answer to the question of the value – or not – of postmodernism in historical study. What we see is a reluctance among practising historians to give up the idea that there is some connection between what they write and past reality, though the cogency of some of the main features of recent theoretical movements is recognized (covertly if not overtly). Iggers summarizes it this way:

There is therefore a difference between a theory that denies any claim to reality in historical accounts and a historiography that is fully conscious of the complexity of historical knowledge but still assumes that real people had real thoughts and feelings that led to real actions that, within limits, can be known and reconstructed. (Iggers 1997: 119)

So what might we conclude from this very brief survey of historical theory and methodology in the past few centuries? No doubt a number of points could be made, but I draw primarily four conclusions:

1. Although put in a particularly stringent manner by the postmodernists and their predecessors such as Nietzsche, the question of epistemology has been discussed at length by historians through the centuries.
2. The idea that past historians worked in a positivistic mechanical fashion, to produce what they saw as objective descriptions of ‘what actually happened’, is a caricature. The history of historical writing is one of self-critique (or at least criticism of the past generation), questioning of rules and attempts to come to grips with an often-difficult profession.
3. The place of the imagination and the subjective insight of the practising historian has been widely recognized through the past two centuries and more. Despite the occasional temporary reign of positivism, the subjective nature of the task and the important contribution made by the individual interpreter have usually been taken for granted.
4. Perhaps one of the most far-reaching developments has been the embracing of the social sciences and the recognition that history should include all facets of life – economics, the structure and complexity of

society, the lives of ordinary people, the material culture, both high and low literature, ideology and beliefs.

1.3.2 Forty Years of Debate Among Biblical Scholars

The survey in the previous section was given as background to the present debate on the history of ancient Israel (for a more detailed discussion of the debate in this section, see Grabbe 2000b). If we go back to the 1960s we find a remarkable consensus on many issues, the differences often largely limited to those between the two schools of Albright and Alt–Noth. Their views on history were available in convenient summary in the histories of Israel by J. Bright (1959 [1st edn]; 1980 [3rd edn]) and M. Noth (1956; 1960). At that time there were two views of the Israelite occupation of the land, the Albright interpretation and the Alt–Noth interpretation of a peaceful infiltration. The Albright interpretation was widely accepted in North America and strongly defended on archaeological grounds by Albright, G. Ernest Wright, Yigael Yadin and others. As far as the ‘patriarchal age’ was concerned, the Albright school accepted that the Genesis account was made up of a variety of sources and could not be taken at face value. Nevertheless, it argued for ‘essential historicity’, again claiming the support of ancient Near Eastern texts. The Alt–Noth school accepted the Wellhausen dictum that the narratives reflected the time when they were written, though Alt still wished to extract data about pre-settlement religion from the narratives.

Already, however, there were signs of changes to come. A little article by George Mendenhall (1962), only sparsely documented and really only a programmatic outline, presented a rather radical reinterpretation of the settlement as a peasant revolt. Then, not long after Albright’s death, a study challenging some of his basic conclusions on the patriarchs was published by T. L. Thompson (1974). Some months later another work on the same subject with similar conclusions, despite a quite different approach, was published by John Van Seters (1975). The death knell to an American consensus was sounded; it was the beginning of a rapid end for the historical patriarchs. The year 1975 also provided what proved to be a diversion in the debate – perhaps more accurately described as a sideshow – with the discovery of the Ebla tablets. Exaggerated claims were made that soon had to be retracted, with much embarrassment (Freedman 1978). This should serve as a cautionary tale to those who leap in to ‘defend’ the Bible by new discoveries that have been studied only incompletely.

Following the rapid fall of support for the patriarchal narratives, the various models of the conquest came under scrutiny. The volume edited by

Hayes and Miller (1977) represented the best of conventional scholarship but was already starting to move in new directions. On the patriarchal period, the knife continued to be driven in (e.g., William Dever's survey of the archaeology in the second millennium illustrated the difficulties for the patriarchal narratives). On the Israelite settlement, Miller's contribution gave a thorough critique of the Albright position and ultimately rejected it (Hayes and Miller 1977: 270–4; cf. Miller 1979). His conclusions present a picture emphasizing the development of Israel from internal populations and also from the Sea Peoples or tribes forced to migrate in their wake, and expresses some scepticism toward the significance of nomadic incursions. Then in 1979 appeared Norman Gottwald's long-awaited study that provided the academic underpinning for Mendenhall's programmatic article almost two decades earlier, and it caused immediate stir and debate. The conquest model was defended by Yigael Yadin in an article published only a couple of years before his death, but this was its swan song (Yadin 1982; cf. Yadin 1979); for practical purposes, it was already dead. A collection edited by Mendenhall and strongly espousing the Mendenhall–Gottwald thesis was published in 1983, though it was notable especially for the trenchant essay by Mendenhall attacking and disowning Gottwald's approach (Mendenhall 1983).

In the 1980s new developments and discussion shift to a much more international arena (much of the debate through the 1970s had been carried on in North America). N. P. Lemche (1985) published a sociological study of Israelite origins that included a major critique of Gottwald, but while differing from Gottwald on many points, including the concept of an internal revolt, Lemche's final picture was of an internal development of the indigenous population. Several new histories of Israel appeared in the mid-1980s. Writing in Italian, A. Soggin (1984) began with David and waited until after dealing with the 'empire under Solomon' to discuss the pre-monarchic period. H. Donner (1984; 1986) completed his history which was firmly in the Alt–Noth tradition. Miller and Hayes (1986) drew on the insights of both North American and Continental scholars. Sheffield Academic Press launched a new series, *The Social World of Biblical Antiquity*, with a number of monographs applying sociology and archaeology to late-second and early-first-millennium BCE Palestine (e.g., Frick 1985).

On the archaeological side, M. Weippert (1979) defended the classic Alt–Noth hypothesis of the settlement, but in an article appearing the next year V. Fritz (1980; cf. Fritz 1981) was already developing a model which looked remarkably like a synthesis of the Alt–Noth and the Mendenhall–Gottwald models, which he referred to as the 'symbiosis hypothesis'. The large-scale archaeological surveys undertaken in Israel brought to bear a whole new

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range of data, and a new generation of Israeli archaeologists began to break through the archaeological tradition strongly influenced by Albright. Israel Finkelstein (1988) appeared in English with a full-blown exposition of his model of the Israelite settlement which delineated a revised version of the original Alt model (but with the full archaeological backing which Alt had never had available) and also drawing on the Mendenhall–Gottwald model. Finally, Helga Weippert (1988) published an archaeological handbook which summarized the results of Palestinian archaeology to the Persian period. The impact of the new archaeological assessment cannot be overestimated, and the discussion gained a whole new basis which outdated all that had gone before.

Through the 1980s the areas of interest began to shift. The settlement and what preceded it remained of intense concern, but now the first part of the Israelite monarchy came firmly into focus. Up until this time, the debate about Israel's origins had concerned the patriarchs and the settlement, with history securely beginning at the reign of Saul. Yet already in 1986 Miller was asking questions about the reliability of the Saul, David and Solomon traditions:

When it first appeared a decade ago, my treatment of Solomon generally seems to have been regarded as overly skeptical, especially in view of what was believed to be strong archaeological evidence in Solomon's behalf. Alan Millard and William Dever were particularly outspoken on this matter. Nowadays, my treatment seems to be viewed as rather traditional and conservative. (Miller 1997a: 12–13)

However, the real shaker was the collection of essays by G. Garbini (1986) whose rapid translation into English facilitated its impact. Garbini was known primarily for his work in Northwest Semitic, rather than biblical scholarship, and a number of his comments arise out of the particular tradition of biblical scholarship in Italy, which look somewhat odd to those in the Germanic/Anglo-Saxon tradition. Yet despite its weaknesses, Garbini's collection of essays attacked some fundamental consensuses in OT scholarship, not least that of the Solomonic kingdom.

Sometimes influenced by Garbini's radical book, but sometimes independently of it, a number of scholars were critiquing the consensus about the Israelite monarchy. R. Coote and K. Whitelam (1987) had done a sociological study which encompassed not only the settlement as such but took in the development of the Israelite monarchy. E. A. Knauf produced two books (1985; 1988) which ignored the emphasis on Israel and instead concentrated on other Palestinian peoples (anticipating the 'Palestinian' arguments of the 1990s – see below), but his critical treatment of all sources

and his scepticism toward the biblical text, until verified, was very much in line with the work of others in this decade. H. M. Niemann (1993) argued that Israel did not become a state until the time of Omri; and Judah, until the time of Uzziah. Diana Edelman (1991; 1996a) did a search for the 'historical Saul'. Perhaps the most influential book, though, was ostensibly an investigation of another subject. D. W. Jamieson-Drake (1991) was asking about the existence of scribal schools, but to answer the question he addressed the issue of when Jerusalem might have become the capital of a major political entity, concluding this was not until the late eighth century.

It would be wrong to assume, however, that the sceptical trend was the only one. Although the abandonment of the patriarchal period and the conquest model was almost universal, the majority of scholars remained within the consensus that we knew a lot about Israelite and Judaeon history at least from the time of David. Among the archaeologists a rigorous debate has been going on for some time between members of the 'Tel Aviv school' (e.g., Finkelstein 1981; Ussishkin 1980; Finkelstein and Na'aman [eds] 1994) and several American archaeologists such as William Dever and Lawrence Stager over the attribution of certain finds to the Solomonic age. However, Dever in particular fronted opposition to those casting doubts on our knowledge of the David and Solomonic age (Dever 1982; 1996; cf. Dever 1998a).

With the coming of the 1990s there was a decade which saw a real explosion of new ideas and challenges to consensuses. Particularly eventful was the year 1992. The *Anchor Bible Dictionary* appeared (though many of its articles were actually completed in the late 1980s). The multiple-author entry on 'Israel, History of' (ABD III, 526–76) will probably become recognized as a classic, with Lemche on the Pre-Monarchic Period, Dever on Archaeology and the 'Conquest', and R. P. Carroll on the Post-Monarchic Period. Also published in 1992 was Thompson's book on the early history of the Israelite people, but the book which in many ways made the greatest impact in that year came from a surprising quarter: this was P. R. Davies' *In Search of 'Ancient Israel'*. I say 'surprising' because Davies made no claim to originality; his aim was to translate and expound some of the recent trends and their basis for students (1992: 7). Yet his book caused a storm: eliciting reviews, such as that by Iain Provan published in the *Journal of Biblical Literature* (1995), along with responses from Davies and Thompson. The publication of the Tel-Dan 'Ben-David' inscription brought a great flurry of claims and counterclaims (§4.1.3.1). G. Ahlström's opus magnum was published posthumously in 1993. The title of Keith Whitelam's 1996 book, *The Invention of Ancient Israel*, with the provocative subtitle of *The Silencing of Palestinian History*, was a harbinger of what has been probably

the greatest stimulus to controversy. In the same year, Lemche brought out a full-scale treatment of the pre-monarchic period (1996a), and the next year a collection of essays on archaeology and history had papers from a number of the protagonists in the debate (Silberman and Small [eds] 1997). The most recent contribution on the issue of the early monarchy is the volume, *The Age of Solomon* (Handy [ed.] 1997), which lays out the main issues and positions.

Unfortunately, the last few years have seen the debate take on a shrillness and *ad hominem* character that it had not seemed to possess before. The long debate between the Tel Aviv and American archaeologists was made by those who differed in a good natured way but had no personal animosity. The sharp exchanges which have come to the surface since the early 1990s are of a different and more ominous quality (e.g., Dever *et al.* 1997). The terms 'maximalist' and 'minimalist' began to be pejorative labels, and the term 'nihilist' came to be used of certain positions (Dever 1995a: 122; cf. 1996 [p. 45] where he clarifies and defends his use of the term, though with evident unease). Some said the 'minimalists' were dangerous; others, that they could be safely ignored. The curious anomaly of dangerous people who can be safely ignored serves as a symbol of the unhelpful way in which the debate has moved. One has the sense that it has ceased to be a matter of academic disagreement and has become an emotive and personal issue.

This is hardly a one-sided fight, however; there has been intimation that those who defended the status quo were nothing but biblical fundamentalists, which has not usually been the case. This has led to a hardening of stances and a tendency to defend established positions rather than debating the issues in a genuine desire to understand the other side. But when things become so ugly that some begin to use the term 'anti-Semitic' of particular academic positions, one begins to despair of any chance of a proper scholarly exchange. It was my frustration over this lack of genuine debate that led to the founding of the European Seminar on Methodology in Israel's History in 1995, with the aim of moving past the 'minimalist'/'maximalist' dichotomy by assembling a group of mature scholars working in the field of history who were willing and able to talk to each other, whatever their positions or disagreements (see Grabbe [ed.] 1997; [ed.] 1998; [ed.] 2001; [ed.] 2003; [ed.] 2005; [ed.] 2007; [ed.] forthcoming a; [ed.] forthcoming b).

Religion is properly a part of any discussion of the history of Israel. Alt, Mendenhall and Gottwald all seem to have been concerned to hold onto the religion in the early traditions while dismissing the history (Lemche 1985: 413–15). Yet the debate in this area has to some extent been carried on in its own way. For over two decades after Georg Fohrer's classic history (1968/1972) little of major significance appeared (Schmidt [1983]

understood his work, strangely, as ‘standing midway between a “history of Israelite religion” and a “theology of the Old Testament”’, while Cross’s [1973] sections on religion had mostly been published sometime before), though a number of relevant individual studies were published (e.g., Alberty 1978). Nevertheless, a great deal of work was being done in individual areas under the stimulus of new discoveries and new questions. Perhaps the most significant new finds were the Kuntillet ‘Ajrud and Khirbet el-Qôm inscriptions which implied that Yhwh had a consort. A perennial question is when monotheism developed in Israel, and also whether aniconism was always a feature of Yhwh worship. Mark Smith’s monograph came out in 1990, written primarily from a Northwest Semitic perspective (2nd edn 2002). This was followed in 1992 by the most thorough synthesis to date, by Rainer Alberty (ET 1994), which made use of all sources, textual, archaeological and ancient Near Eastern. Following Alberty’s work, a plethora of monographs and collections have appeared, and the debate on Israel’s religion is as energetic as that on the history in general (see further at §4.2.5).

1.3.3 Principles of Historical Method Used in this Book

As noted above (§1.1), the present book is not a history as such but an attempt to discuss the issues relating to writing a history (cf. Grabbe 2004a, 13–16). Therefore, the principles that follow do not form a full list of principles for writing a full history; rather they indicate why certain decisions are taken and paths followed in the discussion that follows in subsequent chapters of this book.

1. All potential sources should be considered. Nothing should be ruled out a priori. After a full critical evaluation has been undertaken, some sources might be excluded, but this is only a posteriori.
2. Preference should be given to primary sources, that is, those contemporary or nearly contemporary with the events being described (a concept expounded by L. von Ranke [Iggers 1997: 24; Knauf 1991a: 46]). This means archaeology and inscriptions. The biblical text is almost always a secondary source, written and edited long after the events ostensibly described. In some cases, the text may depend on earlier sources, but these sources were edited and adapted; in any case the source has to be dug out from the present context.
3. The context of the *longue durée* must always be recognized and given an essential part in the interpretation (§1.2.2). One of the factors often

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forgotten is the difference between Israel in the north and Judah in the south. There was a considerable disparity of natural resources and economy between the two, with Judah continually the poorer. There are reasons for this (Lehmann 2003: 149–62; Ofer 1994: 93–5). In Judah the agriculture was largely subsistence and disadvantaged by lack of good soil for grain-growing and rainfall of 300–500 mm per annum. The better soils around Hebron had their value reduced by low rainfall. This meant that pastoralism was an important pursuit. In competition with Israel, Judah definitely came off second best. When Palestine enters the inscriptions of other nations, such as the Assyrians, in the first millennium BCE, there is already a division between Israel and Judah. This long-term division is also hinted at in a number of biblical passages (2 Sam. 2.4; 5.4; Judges 5), in spite of a supposed twelve-tribe nation.

4. Each episode or event has to be judged on its own merits. Even secondary sources have their uses, while primary sources may well contradict one another. Historical reconstruction requires all data to be used, critically scrutinized, evaluated and a judgement made as to the most likely scenario in the light of all that is known.
5. All reconstructions are provisional. New data, new perspectives, new theories may suggest other – better – ways to interpret the data. This openness to new ways of thinking and new configurations of events always needs to be there.
6. All reconstructions have to be argued for. There can be no default position. You cannot just follow the text unless it can be disproved (sometimes expressed in the nonsensical phrase, ‘innocent until proved guilty’ – as if the text was a defendant in court; if there is a forensic analogy, the text is a witness whose veracity must be probed and tested). The only valid arguments are historical ones. Ideology, utility, theology, morality, politics, authority – none of these has a place in judging how to reconstruct an event. The only argumentation allowed is that based on historical principles. Naturally, subjectivity is inevitable in the process, and all historians are human and have their weaknesses and blindspots. This is why each must argue for their viewpoint and then subject the result to the judgement of peers, who are also human and subjective.

Part II

Historical Investigations

CHAPTER 2

Second Millennium: Middle and Late Bronze Ages (2000–1300 BCE)

The Bible begins the story of Israel with creation, or at least with the survivors from the Noachian flood. In many ways Abraham is presented as the first Israelite – even if Israel is made out to be his grandson. This is why most histories of Israel have begun their story sometime in the second millennium BCE, some earlier and some later. The second millennium is a lot of space to cover, but this chapter gives a brief survey, though focusing on those issues that have been associated with writing a history of Israel.

2.1 Sources

2.1.1 Archaeology

The Middle and Late Bronze Ages cover much of the second millennium BCE, MB extending over approximately 2000–1600 BCE, and LB about 1600 to 1200 BCE. These divisions are not exact and are to some extent artificial, but they broadly represent significant differences in culture and society, as well as historical background. As noted earlier, terminology for archaeological periods is not consistent (§1.2.3.2). The scheme used here (cf. Dever 1987: 149–50; Ilan 1998: 297) is:

- MB I (ca. 2000–1800 BCE)
- MB II (ca. 1800–1650 BCE)
- MB III (ca. 1650–1500 BCE)
- LB I (ca. 1500–1400 BCE)
- LB IIA (ca. 1400–1300 BCE)
- LB IIB (ca. 1300–1200 BCE)

These dates are in part based on historical events, whereas the cultures recorded by archaeology do not always follow the historical periods marked off by events. Needless to say, there is much disagreement, with many beginning the LB in 1550 or even 1600 BCE.

Middle Bronze Age I–III has become better known in recent years (Dever 1977; 1987; Finkelstein 1993: 110–31; LaBianca 1997; Ilan 1998). It seems to have been an era of considerable urbanism: one estimate puts the urban population at half as great again as the rural population (Ilan 1998: 305). Many sites were fortified (Finkelstein 1993), and ‘a proliferation of massive fortifications is the single most characteristic feature of the fully developed phases of the period’ (Dever 1987: 153): an estimated 65 per cent of the population lived in a few fortified sites of 20 hectares or more. In MB I an unprecedented surge in settlement swept the hill country, large sites including Shechem, Dothan, Shiloh, Tell el-Far’ah North, Hebron, Beth-zur and Jerusalem (Finkelstein 1993: 117–18). About 75 per cent of the population seems to have lived between Shechem and the Jezreel Valley. In the areas of Ephraim and Manasseh there was a definite extension into the western part of the regions. But this settlement in the central hill country began only later, in MB II. In MB II–III almost every site seems to have been fortified, down to as small as 8–16 dunums.

Dever (1987: 153) suggests that the larger sites were in fact city-states. How one is to relate this conclusion with S. Bunimovitz’s argument that, following the Rank Size Index, the southern coastal plain and the Jezreel Valley were more integrated in MB than LB (1998: 323) is not clear. D. Ilan (1998: 300–1) suggests that the cultural changes coming about in MB were in part caused by immigration of a new population into Canaan (possibly the Amorites and perhaps others such as the Hurrians), even though the ‘diffusionist’ explanation has ceased to be very popular (on the Amorites, see §2.2.1.2). There is evidence of trade with Syria and Mesopotamia, with Hazor as the main ‘gateway’ for Canaan (Ilan 1998: 306–8). There was also extensive trade with Egypt, the main trading centre being Tell el-Dab’a. At first most commerce was with the northern Syrian coast (especially Byblos), but it gradually shifted south. The Middle Bronze Age ended with widespread collapse, often ascribed to the conquest of Avaris and the expulsion of the Hyksos from Egypt, and/or subsequent campaigns by Thutmose III and other Eighteenth Dynasty rulers (Ilan 1998: 314–15). There is now a tendency to see other causes (or additional causes) and also to recognize that the collapse was complex and spread over a considerable period of time. Also, there was considerable cultural continuity with the following LB.

The socio-cultural changes at the end of MB ‘reshaped the social landscape of Palestine and had a profound, long-term impact on Canaanite society’

(Bunimovitz 1998: 320). Although some have seen a major shift away from urbanism in LB, Bunimovitz (1998: 324) argues against this: urbanism was different in scale but the balance between urban and rural remained much the same. The urban centres were considerably smaller but so was the rural sector. A region of major interest, because of its implications for the Israelite settlement, is the highlands: they contained hardly any settlement throughout much of LB (Finkelstein 1988: 339–45). The argument is that pastoralists were the main inhabitants of the hill country on both sides of the Jordan (Finkelstein 1988: 339–45; 1993: 119; 1998c; Ilan 1998: 324). In general the Canaanite city-states were underpopulated and short of manpower (Bunimovitz 1998: 326–7). This is no doubt to be related to the frequent mention in the Amarna letters of the 'Apiru and other groups on the margins of society (§2.2.1.3). The Late Bronze Age ended with a major collapse that seems to have affected the whole of the eastern Mediterranean (§3.1.1).

One issue concerns the cities or sites mentioned in Genesis in connection with the patriarchs (Dever 1977). Albright (1961) had argued that the patriarchal narratives fitted what is commonly called the EB IV period (2200–2000 BCE, though he attempted to date it as late as 1900 or even 1800 BCE). Unfortunately, neither his placement of the patriarchs nor that of others who have tried to put them in the early second millennium BCE can be supported:

A date in MB I [= EB IV or Intermediate Bronze] is ruled out for the patriarchs simply because the latest evidence shows that the main centres traditionally associated with their movements, *pace* Albright, are conspicuously lacking in MB I remains ... To date, not a single MB IIA [=MB I] site has been found in all of southern Transjordan or the Negeb – one of the principal arenas of patriarchal activities in Genesis. (Dever 1977: 99, 102).

At Beersheba (Genesis 21–22) there was a settlement gap between the Chalcolithic and Iron I – no MB remains at all (Herzog 1997; Rainey 1984: 94). Attempts to find the 'cities of the plain' (Genesis 19.24–29) have failed (Dever 1977: 101). It is difficult to find a period in the early or middle second millennium BCE when all sites in the patriarchal narratives were settled; on the contrary, it appears to be not until Iron I that this was so (Clark 1977: 147). On the other hand, many of the main cities known to have existed in MB are completely absent from the patriarchal narratives (Finkelstein and Silberman 2001: 321–3). For further information on the patriarchs, see §2.2.2.

2.1.2 *Egyptian Texts*

The situation in Palestine is known mainly through Egyptian texts. Although the main source is the Amarna tablets, there are various royal and other inscriptions (see the basic collection in *AEL* I–III; also Redford 1992a). Some of these are cited in the discussions below. From a historical point of view, the Merneptah Stela is very important (§3.1.2).

2.1.2.1 Execration Texts

The Egyptian Execration Texts (Seidlmayer 2001; Redford 1992b; *ANET* 328–9; *CoS* I, 50–2) were ceramic objects on which the names of enemies or potential enemies to be cursed were written. The object was then broken to effect the curses. Two sets of broken pieces, dated to the Twelfth or Thirteenth Dynasty (nineteenth or eighteenth centuries BCE) have been found which included names in the Palestinian area. The main value of these is topographical, to show which cities existed in Palestine in particular periods, since only names and no other information is given. Included are apparently sites in Phoenicia (Byblos, Tyre), Syria (Damascus), some sites in Transjordan (such as Pella), but also Aphek, Ashtaroth, Akko, Laish, Hazor, Rehov, Megiddo, Beth-shean, Ekron, Beth-shemesh, Lod and Ashkelon; Shechem and Jerusalem are the only two highland sites in the extant texts. Some personal names also occur, ‘Amorite’ in form, with apparently no Hurrian or Indo-Iranian ones. An argument has been made that the two sets of texts attest to a gradual sedentarizing and urbanizing of the countryside, but this view has been challenged (Redford 1992b). We do know that the Ephraimite hill country, as well as extensive sections of Syria, are absent from the texts.

2.1.2.2 Amarna Letters

Our most extensive information on Canaan comes from the fourteenth century BCE, in the Amarna letters (Moran 1992; *ANET*: 483–90; *CoS* III, 237–42). At this time, Palestine was a part of the Egyptian New Kingdom. The letters belonged to the archive of Akhenaton in the capital of Amarna that he built on a new site. After his death, the capital was abandoned, which is probably why the archive was not eventually discarded. The letters are written in Akkadian which seems to have been the language of international communication at the time. When the city was excavated in the nineteenth

century, among the recovered archives were copies of correspondence between the Egyptian administration and their vassals in other regions. Many of the letters are from local chieftains or city-states in Palestine (including Jerusalem) and tell of the situation there. These so-called 'Amarna letters' give us a unique insight into events in Palestine during this period. Among those who wrote letters to the Pharaoh was the king of Jerusalem (Urusalim). We have six letters from him (EA 285–90), plus a couple of other letters that refer to him (EA 284, 366). The king of Jerusalem is called 'Abdi-Heba. Other kings include Suwardata of Gath and Lab'aya of Shechem. Other cities mentioned are Gaza (EA 129, 287, 296), Ashkelon (EA 187, 320–1, 322, 370), Gezer (EA 253, 254, 287, etc.) and Lachish (EA 287, 288, 328, 329, 335). One term that appears several times is '*Apiru*' (§2.2.1.3).

2.1.2.3 The Story of Sinuhe

Although some have taken this as an actual account of personal experiences, it seems to be a piece of literature (*AEL* I, 222–35; Baines 1982). Its main theme seems to focus on the disadvantages of being removed from one's country of Egypt, but it makes reference to a number of data relating to Syria and Palestine that seem to represent a contemporary description (Rainey 1972).

2.1.3 Ugaritic Texts

The city-state of Ugarit on the Mediterranean coast opposite Cyprus has become quite important for OT study. The city was known about through the Amarna letters but was not discovered until about 1928 in the area of Ras Shamra in Syria. Tablets written in an unknown language and script were unearthed quite quickly. The script was written on clay tablets in wedge shapes but, unlike cuneiform, was clearly an alphabet of 29 letters. The script and language were deciphered within a couple of years, and some of the important tablets were already translated before World War II. Excavations still continue, and tablets have even been found in recent years. The language belongs to Northwest Semitic, the sub-family which also includes Hebrew, Aramaic and Phoenician.

Ugarit already existed as an independent entity in the eighteenth century, but it reached its height during the Amarna period (fourteenth century). We have correspondence between Egypt and Ugarit in the Amarna tablets and in texts from Ugarit. The city was apparently destroyed sometime before 1200 BCE, whether by the Sea Peoples (as often alleged) or others. When the Ugaritic

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texts were first deciphered about 1930, their importance for the mythology and literary world of the Israelites was quickly recognized. Many of the texts are in alphabetic cuneiform and the Ugaritic language (*KTU*; *CML*; Parker 1997). But other texts were in Akkadian and even Hurrian, and many of these have more direct relevance for the history of the eastern Mediterranean in the second millennium BCE. Although Ugarit and the Ugaritic texts have often been used to reconstruct Canaanite culture, mythology and religion, Ugarit seems to have been considered outside of Canaan (Grabbe 1994b). The Ugaritic texts provide some historical information for the Amarna age, though this is usually in the way of general background since they do not usually mention Palestine directly.

2.1.4 Mesopotamian Texts

Most of the Mesopotamian texts do not mention Palestine, though there seems to have been trade between Hazor and Mari (§2.1.1). These texts are important for other sorts of information, and they have been invoked in the past for information relating to the patriarchs. The Mari texts, first discovered in the 1930s, are still being published; a collection of letters in translation is now available (Heimpel 2003). The Nuzi tablets, which brought in the discussion about the patriarchs, are also still in the process of publication (for example, Maidman 1994).

2.1.5 Biblical Text

Our main secondary written source is the book of Genesis. It is really the only biblical book that purports to describe events in the first part of the second millennium BCE. The question is how much the author or compiler knew about the events it describes. Not long ago, it was a strong consensus of scholarship that Genesis was compiled mainly from three sources: the Yahwist (J), the Elohist (E) and the priestly writer (P). Many would still agree with that, but opinion is much more divided. Basically, the old consensus that had developed around the Documentary Hypothesis has gone, though there is nothing to take its place (Rendtorff 1997; Whybray 1987). Some still accept the Documentary Hypothesis in much its original form, but many accept only aspects of it or at least put a question mark by it. There has also been much debate around the J source (Rendtorff 1997: 53–5) and the P source (Grabbe 1997b). It seems clear that the Pentateuch was put together in the Persian period (Grabbe 2004a: 331–43; 2006c). If so, it seems unlikely

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that a substantial memory of second millennium events is to be found in Genesis. True, many traditions in the Pentateuch are accepted to be pre-exilic (for example, Deuteronomy is still widely dated to the seventh century BCE), but that is still a thousand years later than a conventional dating for the patriarchs. An evaluation of the patriarchal tradition on its own terms confirms this a priori position (§2.2.2).

2.2 Analysis

2.2.1 Peoples/Ethnic and Social Groups

A number of different groups are found in our sources, which have at one time or another been connected with the origins of Israel but are also important for the history of ancient Palestine. Here are mentioned those that are important in the second millennium BCE (for the Aramaeans, see §4.2.3). On the general question of labelling such peoples as ‘ethnic groups’, see the discussion at §1.2.4.

2.2.1.1 Hyksos

This was a group of people who became temporary rulers of Egypt but were said to be foreigners associated with Asia (Weinstein 1997a; Oren [ed.] 1997; Redford and Weinstein 1992; Redford 1970b; the monograph of Van Seters [1966] marked a milestone in study of the Hyksos but appeared before major archaeological data were available from Egypt). They ruled during the Second Intermediate Period, making up the Fifteenth Dynasty of the traditional kinglist. One of the main textual sources remains Manetho, as mediated by Josephus and Julius Africanus (Waddell [ed.] 1940), but some native Egyptian sources are also available (Redford 1997a), as is archaeology (Bietak 1997; Redmount 1995). The Egyptian name *ḥq3w ḥ3swt* means ‘rulers of foreign lands’ and seems to have been their name for the actual Hyksos rulers, whereas the people are often referred to as *ʿ3mw* ‘Asiatics’. The names known from scarabs and other written sources appear to be Northwest Semitic.

It was once argued that there was a ‘Hyksos invasion’, part of a large Amorite (§2.2.1.2 below) population movement. It is more commonly argued now that ‘Asiatics’ gradually settled in the eastern part of the delta during the late Twelfth and Thirteenth Dynasties, possibly as slaves or mercenaries. They took over rule to found the Fifteenth Dynasty, with six kings over 108

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years. The question of whether it was a gradual process or a sudden coup is still debated. During the time of their rule there was evidently extensive trade with the Levantine coast, Cyprus and possibly even the Aegean (Betancourt 1997). According to the Egyptian texts, they were expelled into Palestine by Kamose and by his brother Ahmose who founded the Eighteenth Dynasty. Ahmose went on to take the Hyksos centre of Sharuhén (perhaps Tell el-‘Ajjul or Tel Harar). The question of where the Hyksos came from seems to have a firm answer (or partial answer): the archaeology indicates a population largely (at least in the core Hyksos area) made up of those heavily influenced by Canaanite culture (Bietak 1997; McGovern and Harbottle 1997; Redmount 1995). Neutron Activation Analysis indicates that many amphora with imported goods were manufactured in southern Palestine (McGovern and Harbottle 1997), though Bietak (1997: 98–9) continues to argue a connection with the northern Levant, specifically Byblos, because of the architecture.

2.2.1.2 Amorites (Amurru)

The Amorites are often thought to be important for the early history of Israel (Buccellati 1966; 1997; Gelb 1961). The Bible presents them as a pre-Israelite people in the Palestinian region (e.g., Gen. 15.21; Exod. 13.5), sometimes associated with the hill country (Num. 13.29; Deut. 1.7, 44) but also with the Transjordanian region (Num. 21.13, 26). For example, they are mentioned as being among Israel’s ancestors in Ezek. 16.3, though interestingly they were also supposedly one of the peoples opposing Israel on its way to Canaan (Num. 21.21–32). They are known mainly from two sorts of information: (1) their names in cuneiform sources which are Northwest Semitic in structure and different from the Akkadian names (Huffman 1965; Gelb 1980); (2) references to them in cuneiform sources, usually by the Sumerogram MAR.TU. As far as archaeology is concerned, nothing distinctive has been found to relate to them.

From the late third millennium (2500–2000) they are referred to in texts from Ebla and southern Mesopotamia (usually by the label MAR.TU). In the Old Babylonian period (1900–1600), many northwestern names are found and are identified as ‘Amorite’ by their structure, though the individual who bore them is seldom said to be Amorite. They are especially associated with the city-state of Mari on the Euphrates. Amorite tribes include the ‘sons of the south’ (or ‘southerners’) and ‘sons of the north’ (or ‘northerners’). The name of the first attracted attention since it was equivalent to ‘Benjaminites’; however, the reading has been disputed. The last part of the name, *Yamīna*,

apparently referring to the right-hand (or southern) bank of the Euphrates, is clear, but the first part of the name is written in Sumerograms DUMU.MEŠ 'sons (of)'. The question is whether it should be read as Akkadian *mārū* (-*yamīna*) or as Northwest Semitic *bini*(-*yamīna*) (Tadmor 1958a; Anbar 1991: 83–4 n. 324). Gelb (1961: 37–8) suggests that 'sons' is only a semantic indicator of a tribal name and that it is appropriate to refer only to Yaminites. In any case, there is the parallel tribe of DUMU.MEŠ-*si-im-a-al* 'sons of the north' (or 'northerners'). The similarity to the biblical Benjaminites seems coincidental, even if the name is read *Bini-Yamīna* and not *Mārū-Yamīna* or just *Yamīna*.

Many of the descriptions make them nomadic pastoralists, and one text refers to the Amorite as a 'tent dweller [buffeted?] by wind and rain ... the one who digs up mushrooms at the foot of the mountain, who does not know how to ... bend his knee, who eats uncooked meat, who in his lifetime does not have a house, who on the day of his death will not be buried' (Buccellati 1966: 92–3, see also 324–30). Although many of the Amorites were tribal and engaged in pastoralism, this was not true of all of them (Kamp and Yoffee 1980: 89–94). There are texts which refer to Amorites in an urban setting. Also, rather than being seen always as unruly, wild and hostile, they seem to be well blended into Sumerian society. In the Ur III period they are 'fully integrated in every facet of the Mesopotamian social landscape. Amorites were pastoralists, agriculturalists, country dwellers and city dwellers' (Kamp and Yoffee 1980: 98). By the late Bronze Age, references to Amorites (*Amurru*) disappear from the Mesopotamian texts except as a general reference for the region to the west. However, a 'kingdom of Amurru' in Syria is known from references in the Amarna letters, Ugaritic texts and the texts from the Hittite capital at Boğazköy in Anatolia (Gelb 1961: 41–2); some Mari texts suggest that it already existed several centuries earlier than the Amarna period (Gelb 1961: 47). It was located west of the Euphrates, in Syria, between Lebanon and Damascus; apparently a section of the territory extended as far as the Mediterranean (Gelb 1961: 42).

There are disagreements over Amorite origins. In the past it has been common to label them nomads on the fringes of civilization in the Euphrates region, and some texts seem to give support to this picture. Others think they were originally farmers in a narrow region of the mid-Euphrates who expanded their territory by moving into the steppe and taking up sheepbreeding. Some cuneiform texts consider them as uncultured, semi-wild people of the wilderness, but this is mainly a biased view. Their names are important because Semitic names had meaning, being sometimes a complete sentence (e.g., 'God has blessed'), which allows the grammar of the language to be partially reconstructed by names alone. Their language was Northwest Semitic and in the same language family as Hebrew, Aramaic, Phoenician

and Ugaritic. This suggests a closer affinity with these people, at least in their origins, than to the eastern Assyrians and Babylonians.

2.2.1.3 'Apiru/Haberu

One term that appears several times in second millennium texts is 'Apiru' or possibly 'Hapiru' or 'Haberu' (Na'aman 1986c; Lemche 1992a; Gottwald 1979: 397–409; Loretz 1984, but see the review of Na'aman [1988b]), often written in cuneiform with the Sumerograms SA.GAZ; Ugaritic: *'pr*; Egyptian *'prw*. When these texts were first studied a century or so ago, it was assumed that it was an ethnic term related to 'Hebrew'. Many modern scholars agree that the term 'Apiru and Hebrew are cognate, but that neither was originally an ethnic term but a social designation: the word seems originally to mean someone outside the social system or an outlaw (Gottwald 1979: 401, 404) or a refugee or migrant (Na'aman 1986c; Lemche 1992a). In the early texts it appears to have a merely descriptive meaning of 'migrant'. People were always temporarily in this category because they would soon be integrated into the (new) society and location. Yet migrants often took on employment that might be considered marginal by the natives, such as becoming mercenaries. Or on occasion they might become brigands as the easiest or even the only way to survive.

In the Amarna letters many of those labelled 'Apiru seem to have sold themselves as mercenaries to the highest bidder, while others turned to raiding or stealing. Therefore, the term not infrequently has a pejorative connotation along the lines of 'outlaw' or 'bandit', and was used of one's enemies, regardless of whether they were truly 'Apiru (EA 68; 185; 186). In some cases, the writer accuses fellow kings of city-states of siding with the 'Apiru or employing them against the Pharaoh's interests (EA 286; 287; 288, 289) or asserts that the 'Apiru would take over (EA 366) or even that the rulers themselves are becoming 'Apiru (EA 67; 288). In the biblical text the word has become an ethnic term, used by Israelite and Judahite writers only for themselves, or by outsiders such as the Philistines, perhaps as a way of satirizing the outsiders (Na'aman 1986c). In some passages in the laws, however, it seems to have much of the original base meaning of one who was likely to be vulnerable and poor and in need of legal protection, perhaps even a slave (Exod. 21.2; Deut. 15.12; Jer. 34.9, 14). Idrimi, king of Alalakh, joins 'Apiru when he himself becomes a refugee:

An evil deed happened in Halab, the seat of my family, and we fled to the people of Emar ... (So) I took with me my horse, my chariot, and my groom, went away and crossed over the desert country and even entered into the region of the Sutilian warriors ... but the next day I moved on and went to the land of Canaan. I stayed in Ammia in the land of Caanan [*sic!*]; in Ammia lived (also) natives of Halab, of the country Mukishkhi, of the country Ni' and also warriors from the country Ama'e ... There I grew up and stayed for a long time. For seven years I lived among the Hapiru-people. (ANET 557; Greenberg 1955: 20)

2.2.1.4 Shasu (Shosu, Š3šw, Sutu)

A group referred to in a number of Egyptian texts (Giveon 1971) are the Š3šw, usually transcribed as Shasu or Shosu (Redford 1990: 68–75; 1992a: 269–80; Weippert 1974; Ward 1972; 1992); Akkadian texts, such as the Amarna tablets, seem to refer to the same group as *Sutu* (EA 16; 122; 123; 169; 195; 297; 318). They are often associated with the area of southern Transjordan in Egyptian texts (Redford 1992a: 272–3). Seti I conducted an Asian campaign in which he defeated the Shasu from the fortress of Sile to (the city of) Canaan (= Gaza?) (ANET 254). In a frontier report from the Papyrus Anastasi VI (ANET 259; Giveon 1971: #37) the Shasu tribes are allowed to pass the fortress with their cattle. Several texts mention geographical areas associated with the Shasu (Giveon 1971: ## 6a; 16a): 'the land of the Shasu Samath' (*t3 š3šw smt*; *šsw smt*), 'the land of the Shasu Yahu' (*t3 š3šw Yh[w]*; *šsw yhw*), 'the land of the Shasu Trbr' (*t3 š3šw trbr*); 'Seir (in the land of) the Shasu' (*šsw s'rr*); 'Laban (in the land of) the Shasu' (*šsw rbn*); *šsw psps* 'Pyspys (in the land of) the Shasu'; *šsw wrbw* 'Arbel (?) (in the land of) the Shasu'. Ramesses II claimed to have destroyed the land of the Shasu and captured the mountain of Seir (Giveon 1971: #25). Reference is also made to the tribes of the Shasu of Edom (Giveon 1971: #37). This locates the Shasu in the area of Edom, Seir and Transjordan east of the Arabah.

But the question is complicated by the fact that Shasu are mentioned in lists that include toponyms from other areas, some as far away as Mesopotamia (Giveon 1971: ## 4, 5, 6, 7, 48). Also, Ramesses II's version of the battle of Qadesh refers to the capture of two Shasu who are spying for the Hittites (Giveon 1971: #14). None of these examples proves that Shasu came from other regions, since the topographical lists have diverse names, and the spies accompanying an army would not necessarily remain in their home territory. But another list seems to include mainly names from northern Palestine or northern Syria (Giveon 1971: #13; cf. #12); however, since not all sites can be identified, it is not decisive. From the data so far known, it seems most

likely that the Shasu are associated with a specific area around the southern and eastern part of the Dead Sea (Redford 1992a: 272–3). If this is correct, it undermines Ward's statement (1992) that the Shasu represent 'not an ethnic group but rather a social class'. His is a peculiar assertion since a social class is not usually said to be a people and to have their own country, while the text and the normal determinative used with the Shasu indicate both a people and a territory.

Perhaps the hottest debate has to do with whether the Israelites arose from the Shasu. Such has been proposed by Rainey (2001) and Redford (1990: 73–5; 1992a: 267–80). This is partly based on the reliefs associated with Merneptah's supposed conquests (§3.1.2). Some argue that the 'Israel' of the inscription is to be identified with the Shasu of the reliefs. In my view, this goes far beyond the evidence. The equation of the reliefs with entities in the inscription is more ingenious than convincing. In any case, it seems that the texts distinguish the Israelites, Canaanites and Shasu. This does not mean that both Shasu and Canaanites could not have joined the settlers who became Israel, but once Israel had its own identity (as indicated by the Merneptah inscriptions) it was seen as separate from both the Canaanites and Shasu. See further the discussion in relation to the Merneptah inscriptions (§3.1.2).

2.2.1.5 Canaanites

In the biblical text the 'land of Canaan' is the common way of referring to the area on the western side of the Jordan; similarly, the 'Canaanites' are the inhabitants of this region. The Canaanites (sometimes listed as several different tribes [see below]) are the traditional enemies of the Israelites and also the bad example of the traits and practices that they are to avoid (Lev. 18.3; Deut. 12.29–31; 18.9). Lemche (1991) makes a good case for the biblical picture of the Canaanites being a literary construct, based on certain ideologies. If he is correct, one cannot rely on the Bible for information on the historical Canaanites. Lemche goes on to argue that in other sources as well the term 'Canaan/ites' had no precise geographical or ethnic content. Contrary to Lemche, however, original sources from the second millennium BCE and elsewhere indicate a geographical content to Canaan and Canaanite that is as specific and meaningful as many other such names in the texts (Na'aman 1994b; Hess 1998; Killebrew 2005: 93–148; Rainey 1996; cf. the response in Lemche 1996b; 1998c). I see no difference in the many references in the Amarna letters to other geographical or ethnic entities. Many passages are not very specific, of course, but a 'passport' from the king of Mitanni to the 'kings of Canaan' to allow his messenger to pass (EA 30) must have had

some practical purpose. Similarly, a number of the texts available are legal texts, such as a note of indebtedness in which the debtor is identified as a ‘man of Canaan’ (LÚ URU *Ki-in-a-ni*^{kl}; Rainey 1996: 3; Na’aman 1999: 32).

Lemche (1991: 152) makes the point that no one would have referred to himself as a Canaanite. Whether that is true is debatable (cf. Na’aman 1994b: 408), though it might be true if the term was primarily a geographical designation. But there are many examples in history of a group of people who are known mainly by a name – even a pejorative name – given to them by outsiders (for example, the names for the Sioux Indians and the Eskimos in North America meant ‘enemy’). The present-day trend to call aboriginal groups by their own designation does not negate the fact that the group had an identity – even an ethnic identity – in spite of the use of a name that they themselves might have rejected. Also, we do not have to know precise borders to a geographical area or territory before the name has meaning. How many of us could give a precise delineation of the Sahara, even though we all know basically what it refers to?

From the indication in the Egyptian, Ugaritic and Mesopotamian sources ‘Canaan’ referred to what we call Palestine and Phoenicia (Killebrew 2005: 94). Within that territory was a variety of ethnic groups. Perhaps all the inhabitants of this region had a tribal or ethnic name for themselves, in which case ‘Canaanite’ was an outsider’s term for any inhabitant of the region. If so, our term ‘Canaanite’ as a contrast with ‘Israelite’ is nonsense: Israelites were as much Canaanite as anyone else (Grabbe 1994b). Most of the references to Canaanite/Canaanites seem to be geographical and support Killebrew’s decision to use it as a purely geographical term. Yet the term seems to be ethnic in the biblical text (and this might also be the case in a few ancient Near Eastern passages). It is easy to explain this as a misunderstanding, especially in the light of other biblical distortions with regard to the historical Canaanites. But there is enough imprecision in our data to make us back away from dogmatic statements. ‘Canaan/ite’ had meaning in antiquity, but whether we yet have the precise usage pinned down might still be debated.

The biblical text’s references to Canaanites sometimes appears to be generic, including all the inhabitants of the land of Canaan (Gen. 12.6; Deut. 11.30; 21.1; Josh. 3.10; 5.1; 17.16, 18; Judg. 1.9-17, 27-33). At other times, the Canaanites seem to be just one of a number of peoples living in the land: Kenites, Kenizzites, Kadmonites, Hittites, Perizzites, Rephaim, Amorites, Canaanites, Girgashites, Jebusites (Gen. 15.19-21); Canaanites, Hittites, Amorites, Perizzites, Hivites, Jebusites (Exod. 3.8; 23.23; 34.11; Deut. 20.17; Josh. 9.1; Judg. 3.5); Hittites, Girgashites, Amorites, Canaanites, Perizzites, Hivites, Jebusites (Deut. 7.1; Josh. 24.11); Canaanites, Hittites,

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Perizzites, Jebusites, Ammonites, Moabites, Egyptians, Amorites (Ezra 9.1). Also of interest are the Horites, though these are associated with Seir/Edom and do not feature in the other lists (Gen. 14.6; 36.20-30; Deut. 2.12, 22). How are we to understand these lists? Na'aman (1994c: 239–43; 1994d) argues that a major displacement of peoples occurred at the end of LB, at which time a number of 'northerners' migrated into Palestine, including a number of peoples from Anatolia, such as the Hittites, Hivites, Girgashites and Jebusites (the Perizzites are so far unattested outside the Bible). If so, far from being enemies of Israel, they may have been one of the constituents of the developing ethnic group in the Palestinian highlands (§3.3).

2.2.2 *Question of the Patriarchs*

The question of the narratives about the 'patriarchs' – those who are made the ancestors of Israel in Genesis – was strongly debated in the mid-twentieth century. A variety of views was advanced. Perhaps the most exotic was that of C. H. Gordon (1958; 1963) who argued that Abraham was a 'merchant prince' who lived in the Amarna period, for which he found parallels in the Ugaritic and other ancient Near Eastern texts. Alt and Noth had followed Wellhausen's dictum that the contents of the texts reflected the history of the age in which they were composed. Alt's interest in the patriarchal texts was mainly in the religion reflected there (cf. Alt 1966). But the Albright school was particularly effective in promoting the idea that the patriarchal traditions contained 'substantial historicity'; the following statement by J. Bright is exemplary:

When the traditions are examined in the light of the evidence, the first assertion to be made is that already suggested, namely, that the stories of the patriarchs fit authentically in the milieu of the second millennium, specifically in that of the centuries sketched in the preceding chapter [twentieth to seventeenth centuries BCE], far better than in that of any later period. The evidence is so massive and many-sided that we cannot begin to review it all. (Bright 1980: 77)

The basic problem was that the only information preserved was what could be found in the text of Genesis – there was no direct external confirmation, either epigraphic or literary. A number of further problems presented themselves, such as the following:

1. Except for Jacob/Israel the references to the patriarchs are attested in Israelite tradition only late. Apart from the Genesis texts, Abraham

- (1 Kgs 13.36; 2 Kgs 13.23; Isa. 29.22; Micah 7.20) and Isaac (1 Kgs 18.36; 2 Kgs 13.23; Amos 7.9, 16; Jer. 33.26) are little mentioned. R. E. Clements expressed the view that 'in the preexilic prophets, there is no authentic reference to the Abraham traditions. Mic. 7:20 is a postexilic oracle, as is also probably Isa. 29:22' (*TDOT* I, 57).
2. The patriarchal narratives in Genesis in their present form reflect a later time, with many anachronistic details: the Philistines are in the land long before the migration of the Sea Peoples; Arameans (Gen. 22.21; 24.10) who are first attested about 1100 BCE in an inscription of Tiglath-Pileser I; Arabs who first occur about the ninth century (Gen. 26.12-18); the Chaldeans (Gen. 11.28) are attested after 1000 BCE but are mainly important in the Neo-Babylonian period (Ramsey 1981: 40-2) – while the migration from Ur has an interesting parallel to the return from exile. There has been something of a debate over the presence of camels in Genesis: Albright had argued that this was anachronistic and made Abraham a donkey caravanner, but Gordon and also some conservatives had claimed evidence for the domestication of camels at an earlier time. The most recent evidence for the domesticated camel in Palestine, however, seems to be no earlier than the Iron Age, with the concentration of bones at Tell Jemmeh, apparently a caravan centre, focusing on the seventh century BCE (Wapnish 1997; Zarins 1978; Na'aman 1994c: 225-7; Finkelstein and Silberman 2001: 37).
 3. Archaeology has sometimes been drawn on in support of 'substantial historicity', but the most recent study is mainly negative (see §2.1.1 above).
 4. Chronology is a significant issue: if the patriarchs are historical, when did they live? If the narratives are reliable, reliable for when? Some writers act as if the chronology can be taken for granted, but it cannot be. Having asserted how well the narratives fit the early second millennium (as quoted above), Bright then went on to admit:

Granting the above, does the evidence allow us to fix the date of the patriarchs with greater precision? Unfortunately, it does not. The most that can be said, disappointing though it is, is that the events reflected in Gen. chs. 12-50, for the most part, fit best in the period already described, i.e., between about the twentieth and seventeenth centuries (MB II). But we lack the evidence to fix the patriarchs in any particular century or centuries and we have, moreover, to face the possibility that the patriarchal stories combine the memory of events that took place over a yet wider span of time. (Bright 1980: 83)

In fact, a number of other scholars dated the patriarchs many centuries later than Albright and Bright: later in MB, in LB and even in Iron I (Dever 1977: 93–6; Clark 1977: 143–8).

5. Mode of life. It was once assumed that the patriarchs were nomadic and that this was uniquely in line with the early-second-millennium BCE context (Ramsey 1981: 34–6). Much discussion has taken place in the past few decades, undermining this argument (§3.2.4.3).
6. Names. Many parallels can be found to the names in the patriarchal narratives. Bright asserted that they fitted the early second millennium, but a number of his examples actually came from later than the first few centuries of the second millennium. Names cannot be proof, of course, because the patriarchal names can all be found in the telephone book of almost any large Western city today; however, it is interesting that a number of the names do not recur in the Israelite tradition until the Graeco-Roman period. But as Thompson (1974: 35) points out, most of the names are typical Northwest Semitic in structure. Some of the names for Abraham's ancestors in Gen. 11.20–32 are actually topographical names in the region of Haran, as known from Mesopotamian texts (Schneider 1952: Serug, Nahor, Terah, Haran). On the name Benjamin, supposedly known from the Mari texts, see §2.2.1.2.
7. Customs have been one of the main pieces of evidence. A good example of this is E. A. Speiser's commentary on Genesis (1964). Drawing extensively on the Nuzi texts, he used them to illustrate many passages in the patriarchal narratives. Yet at times his argument was not that the biblical custom could be found in the Nuzi text but that the custom from the Nuzi text was not understood by the biblical writer – a rather strange way of arguing for authenticity and reliability. In fact, many of the customs are not parallel to the biblical passage, or either the Nuzi text or the biblical text have been misunderstood or misrepresented: for example, the idea that Eliezer was an heir because he was an adopted son of Abraham but would not inherit once Isaac was born was actually contrary to the Nuzi custom (Thompson 1974: 203–30; Donner 1969). Abraham's and Isaac's passing of their wives off as sisters (Gen. 12.10–20; 20.1–2; 26.1–11) was said to reflect a Hurrian custom of adopting the wife as a sister. The biblical text does not actually suggest such an adoption (Speiser argued that the Genesis writer no longer understood this custom), but the Nuzi practice was actually misapprehended by some modern scholars (Greengus 1975; Eichler 1977). The best parallel to Genesis 23 seems to come from the Neo-Babylonian period (Tucker 1966; Petschow 1965). In the end,

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none of the alleged customs demonstrating an early-second-millennium background for the patriarchal stories seems to have stood up.

2.3 Synthesis

The story of Israel and Judah began as such only at the end of the second millennium BCE. Yet a full understanding requires a knowledge of the history of Palestine throughout the second millennium and even more broadly the history of the ancient Near East. Only a brief outline can be given here, the main sources being the *Cambridge Ancient History* (CAH), Klengel (1992), Kuhrt (1995) and Van De Mieroop (2004). In the discussion that follows I shall divide treatment into early and later second millennium, but the line dividing the two is really a broad band stretching from about 1600 to about 1500 BCE.

The Middle and Late Bronze Ages cover much of the second millennium BCE. Approximately 2000 BCE marks the end of the Early Bronze IV (or Intermediate Bronze, in some terminology) and the beginning of the Middle Bronze but also the end of the Ur III Period and beginning of the Old Assyrian Period in Mesopotamia and the end of the First Intermediate Period and beginning of the Middle Kingdom in Egypt. Then the point of about 1600/1500 BCE marks the transition to the Late Bronze Age and also the end of the Second Intermediate Period and beginning of the New Kingdom in Egypt, and the beginning of the Hittite Old Kingdom. The late Bronze Age is usually seen as ending about 1200 BCE, though some have argued that it should be later, perhaps in the latter half of the twelfth century.

Chronology for the second millennium BCE is often not certain (Van De Mieroop 2004: 4). There are a few astronomical dates, which are very precious but not very frequent. Events can also be co-ordinated by linking them to the same event as recorded or mentioned in other regions. For the most part, there are only a few fixed dates, with everything else fitted around them. Basically, the earlier an episode the less certain its dating. Over the years, three basic chronological schemes have been proposed, a high, middle, and low chronology. In Mesopotamia, it all depends on the dating of Hammurabi. In the early second millennium, up to 50 or more years separate the higher from the lower chronology; in the late second millennium, the differences are more like a decade (see further Dever 1990; Weinstein 1991; 1992; Hoffmeier 1990; 1991).

About 3000 BCE writing seems to have originated in both Egypt and Mesopotamia, beginning the historic period for both regions. The Egyptian priest Manetho, writing in Greek under Ptolemaic rule about 300 BCE,

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divided Egyptian history into 30 dynasties (Waddell 1940). Although it is now recognized that Manetho's divisions were not always justified and some of these dynasties were contemporaneous rather than consecutive, the dynasties became the framework for writers on Egyptian history even to the present, despite better sources for certain periods. According to Manetho, the first real king of Egypt was Menes who united Upper and Lower Egypt about 3000 BCE and initiated the First Dynasty. The Early Dynastic Period consisted of Dynasties 1–2 (ca. 3000–2700 BCE). The Old Kingdom covered Dynasties 3–6 (ca. 2700–2150 BCE), during which time most of the pyramids were built. The Old Kingdom ended with what is called the First Intermediate Period (ca. 2150–2050 BCE), a time of turmoil which Egyptian writers, with their emphasis on order, found particularly abhorrent. This included the short-lived Dynasties 7 and 8, when order (in the sense of strong central government) began to break down, and then Dynasties 9–10, and finally Dynasty 11 during which central control was re-established.

The first civilization in Mesopotamia was in the southern region of Sumeria. The Sumerians spoke a language not related to any other known language, and it was for this language that the writing system on clay tablets was developed. This was originally pictograms, but it soon became clear that it was easier to outline the figures by jabbing the triangular point of the stylus into the clay than to try to draw with it. Thus, the pictograms soon evolved into characters made up of wedge-shaped marks; hence, the name *cuneiform*. Sumeria as a power came to its peak with the Ur III period (ca. 2100–2000 BCE), when the city of Ur dominated the region. After that the history of Mesopotamia belongs mainly to the Semitic peoples further north, known as Akkadians (from the city Agade or Akkad), a collective term for both the Assyrians in the north and on the Tigris and the Babylonians on the Euphrates. They adopted Sumerian culture (much as the Romans adopted Greek culture), including the writing system even though it was not well designed for writing a Semitic language such as Akkadian. Texts in the Sumerian language continued to be copied and even new ones written well into the first millennium BCE, but Sumerian was probably a dead language during most or all of this time and used only for literary purposes, much as Latin was in the Middle Ages.

2.3.1 First Part of Second Millennium (ca. 2000–1600/1500 BCE)

2.3.1.1 Egypt

The Middle Kingdom (ca. 2050–1700) was made up of Dynasties 11–12. The Twelfth Dynasty was one of the most famous, reaching a height of

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power and culture. However, it ended in the Second Intermediate Period (ca. 1700–1550). This encompassed Dynasties 13–17. The length of the individual dynasties is uncertain, but some of them were contemporaneous. In many cases we have nothing more than the names of the kings within the dynasties, though even here there are questions. The Second Intermediate Period was dominated by the Hyksos (§2.2.1.1).

2.3.1.2 Old Assyrian Period (ca. 2000–1750)

With the collapse of Sumerian power, the Assyrians and Babylonians tended to dominate Mesopotamian history for the next 1500 years until the Persian conquest. The Assyrians were known much of this time for their military prowess, but early in the second millennium they conquered through trade, by establishing a trading empire in Anatolia. This was done by merchants (rather than government) who set up trading colonies in different cities by agreement with the city rulers (Larson 1976). Charters were drawn up giving the merchants certain rights, some of which have been preserved. The trade was mainly in textiles and tin (needed to make bronze, the principal metal of this time). One of the figures who promoted this sort of trading empire was Shamshi-Adad I (ca. 1800 BCE) who was an older contemporary of Hammurabi and (like Hammurabi) of Amorite ancestry (see §2.2.1.2 on the Amorites). He also conquered Ashur and brought the variety of Assyrian city-states under his control, but the empire collapsed after his death. For the next two or three centuries, we know little about Assyria.

2.3.1.3 Old Babylonian Period (2000–1600)

The height of Babylonian greatness came in the early second millennium, not to be equalled until the brief Neo-Babylonian empire (605–539 BCE). There were several rival states (Isin, Larsa and Babylonia), but the area was united by Hammurabi of Babylon. The date of his reign is very important for the chronology of this period but varies widely between those advocating the high, middle, or low chronology: his dates vary from ca. 1850–1800 to 1800–1750 to 1730–1680 BCE. Hammurabi conquered many cities of the region, including the city-state of Mari on the Euphrates. After the death of Hammurabi, Babylonia declined over the next 150 years. It was finally devastated by a Hittite attack under Mursilis I about 1600 BCE. This weakened the state considerably and allowed it to be taken over by the Kassites, usually thought to have come from the Zagros mountains. The

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Kassites are usually seen in much the same way as the barbarian tribes who destroyed Rome. Babylonia was controlled by the Kassites for the next three centuries until about 1150.

2.3.1.4 Hittites

The original inhabitants of Asia Minor were a group often referred to as the Hatti. During the early second millennium BCE an Indo-European people, the Hittites, infiltrated the country and became the ruling class. They developed their own writing system, Hittite hieroglyphic but also used Akkadian cuneiform writing. It is uncertain how the empire developed (compare the situation in the Old Assyrian period). The Hittite Old Kingdom straddled the line between the Middle and Late Bronze Age (ca. 1650–1500). A number of historical texts have survived from this period. Mursilis I conquered Aleppo and Babylonia, opening the way for the Kassite invasion; however, palace revolutions and Hurrian pressure weakened the state.

2.3.1.5 Northern Syria

The sources for Syria are limited, but there are two main ones: the Mari texts and the Alalakh tablets. Mari was a city-state on the Euphrates in northern Syria. It has become quite famous because excavations beginning in the 1930s have turned up many texts of diverse sorts, including prophetic texts that predicted the defeat of Hammurabi. The kingdom was ruled by various Amorite dynasties. Alalah was further west. About 500 texts were found in the excavations of the city, mainly from two periods: about 1700 BCE and the fifteenth century. They are mostly lists of various sorts: inventories, rations, landholdings, loans, etc. They thus tell us mainly about social, legal and economic aspects of the people, though there are some documents about historical events. We also know something about the city-state of Yamḥad (modern Aleppo). There are only scattered references, but these indicate a substantial state during the Old Babylonian period, strong enough to resist the moves of Shamshi-Adad I to take control. The city of Yamḥad was destroyed by the Hittite king Mursilis I about 1600 BCE.

A group of people known from Mari and northern Syria – but also in central Mesopotamia – were the Amorites; on them see §2.2.1.2.

2.3.2 Second Part of the Second Millennium (1600/1500–1200 BCE)

2.3.2.1 Egypt

The Egyptian New Kingdom, consisting of Dynasties 18–20 (ca. 1550–1050 BCE), was a high point in the history of Egypt. Dynasty 18 was responsible for the ‘expulsion’ of the Hyksos (see §2.2.1.1 for the complications to what had happened). They first secured their northern and southern borders and then began to expand into Nubia and Syro-Palestine. This brought them into conflict with the Hittites and the kingdom of Mitanni in the north. Eventually Thutmose IV (ca. 1400 BCE) reached an agreement with Mitanni. One Pharaoh of the Nineteenth Dynasty is quite famous because of being a woman. When Thutmose II (ca. 1500 BCE) died with a minor son, his wife took the throne and ruled as Hatshepsut (Tyldesley 1996); she apparently took on the persona of a man in certain contexts and is sometimes so represented. The son reached maturity, and took the throne as Thutmose III at Hatshepsut’s death. One of her descendants was Amenophis IV (1360/1350–1345/1335 BCE) who is one of the most famous of the ancient Egyptians (Redford 1984). The priesthood and temples were dominated by the cult of Amen Re, but Amenophis promoted the cult of Aten (the sun disc). This ‘religious reform’ is still debated by modern scholars, though the idea that it was the first example of monotheism is probably exaggerated. Amenophis changed his name to Akhenaten and built an entirely new city as his capital, what is now Tell el-Amarna, giving the name ‘Amarna period’ to the mid-fourteenth century BCE. After Akhenaten’s short reign of 16 years, his name was blackened and even erased from inscriptions and monuments. The powerful priesthood of Amen Re seems to have been partly behind this. In any case, his capital was abandoned, leaving a unique find for archaeologists when it was excavated in the late 1800s (because it was built on a new site and nothing was built on top of it). Of particular importance were the archives abandoned there, the Amarna letters (§2.1.2.2).

When the last king of Dynasty 18 had no offspring, he appointed his vizier as his successor: Ramesses I who initiated Dynasty 19 (ca. 1300 BCE). Perhaps the most famous Pharaoh of this dynasty – and one of the most famous of all the Pharaohs – was Ramesses II (1290/1280 BCE for 66 years) (James 2002). The expansion of the Hittites had brought them and Egypt into conflict. A famous battle (still studied by military strategists) took place at Qadesh in northern Syria, with both sides claiming victory. Ramesses then concluded a treaty with the Hittites. (It was once common to make Ramesses the Pharaoh of the exodus, which is rather strange considering that far from being destroyed, Egypt was at its height under his reign; on the exodus see

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§3.2.1.) His son was Merneptah who set up the victory stela known as the Merneptah Stela or Israel Stela (§3.1.2). Merneptah apparently had to put down a number of rebellions in Palestine and elsewhere.

The Twentieth Dynasty (ca. 1200–1050 BCE) saw the close of the New Kingdom. It was during the reign of Ramesses III (1180–1150 BCE) that the Sea Peoples were supposed to have invaded (§3.2.2). The situation is probably more complicated, but Ramesses III may well have fought a sea battle or even a sea battle and a land battle with them. Toward the end of the Twentieth Dynasty, Egypt became divided into north and south, and control over Palestine was gradually lost (probably ca. 1130 BCE [§3.1.1]). It was succeeded by the Third Intermediate Period which lasted 400 years until the Saite period (664 BCE).

2.3.2.2 Mesopotamia

The period of Kassite rule in Babylon was a lengthy one (1600–1150 BCE). Not much is known, though the older view that the Kassites were seen as foreigners and illegitimate probably requires revision. Interestingly, the rulers were designated as ‘kings of Babylonia’, that is, the whole region, whereas previous kings had been kings of a particular city-state. The Amarna archives contain communications between Egypt and Babylonia, indicating that Kassite Babylon was an international power. Kassite rule was weakened by conflict with the Assyrians, and rule of Babylonia was replaced by rule from the city of Isin (Second Dynasty). The Aramaean incursions caused problems, as did Elam (a power in the area later to be settled by the Persians). Nebuchadnezzar I (ca. 1100 BCE) defeated Elam and brought a revival of Babylonia’s fortunes for a time.

In Assyria there was a 400-year ‘dark age’ from about 1750 BCE to the beginning of the Middle Assyrian period (ca. 1350–1050 BCE) during which time we have little information on Assyria. The expansion of Mitanni had brought about the loss of Assyrian independence (ca. 1400 BCE), but the defeat of Mitanni by the Hittites gave Assyria its chance to regain independence and begin to exert itself again. The Assyrians took some territory from Mitanni, including the region of Nineveh, and eventually Mitanni itself (ca. 1300 BCE), now setting their borders directly on the Hittite Empire. Contacts were re-established with Egypt (known from the Amarna archives), and the Babylonians began to worry about the new Assyrian strength as the latter shifted the boundary between the two powers further south. It was not long before the Assyrians took control of Babylon for a time (ca. 1200 BCE). Toward the end of this period the Aramaeans

became a threat by launching raids on Assyrian territory and even taking some of it temporarily (§4.2.3).

2.3.2.3 Hittite Empire (ca. 1400–1200 BCE)

The term ‘Hittite Middle Kingdom’ was once used but has now generally been rejected as a concept. The Hittite empire is also known as the ‘Hittite New Kingdom’. As the Hittite strength grew, Egypt and Mitanni attempted to counter it. The powerful Šuppiluliumas I (ca. 1350 BCE) conquered northern Syria and made Mitanni into a protectorate. An attempt to establish good relations with Egypt was thwarted, however. The conflict with Egypt culminated in the battle of Qadesh (ca. 1300 BCE) between Ramesses II and Muwatallis II, leading to a treaty between Ramesses and Muwatallis’s successor Hattusilis III (ca. 1275 BCE). The kingdom seems to have come to an end by internal decline rather than being destroyed by the Sea Peoples as was once thought. It was succeeded by a series of Neo-Hittite states that carried on for another 500 years.

2.3.2.4 Mitanni Kingdom (ca. 1600–1350 BCE)

The kingdom of Mitanni was once thought to be composed of two different populations. The main group were the Hurrians, a people who penetrated into northern Mesopotamia and Syria in the early second millennium BCE. We know that Hurrians were found among the populations of many cities across Asia Minor and northern Syria, though most of the penetration into Syria came after 1500. Their leaders were believed to be Indo-Aryan, some of the ancestors of the migration of people from southern Russia into Iran and northern India. Their names and some of the few words of their language were also thought to support this. One word borrowed into many languages across the region was *maryannu* ‘charioteer’, a word supposedly known from Sanskrit. However, although Indo-Iranian influence can be demonstrated, it is now thought to have been exaggerated, and claims are now made that the word *maryannu* is Hurrian in origin. The kingdom of Mitanni was made up of a union of Hurrian and Amorite states. As it expanded it clashed with Egypt, but it concluded a treaty with Thutmose IV about 1400 BCE. The Hittite king Šuppiluliumas made Mitanni into a protectorate (ca. 1350 BCE), which allowed Assyria to gain independence and expand. Not long afterward (ca. 1300 BCE), Mitanni was destroyed by the Assyrians who took control of their territory, despite its being a supposed Hittite possession.

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Table 2.1

CHRONOLOGY OF THE ANCIENT		
3000		First writings in Egypt and Mesopotamia
2500		City-state of Ebla
2000	Middle Bronze I (2200–2000)	End of Ur III Period (ca. 2000)
.....		
	Middle Bronze IIA (2000–1750)	
1900		Old Assyrian Period (2000-1800) Shamshi-Adad I (ca. 1800)
1800	Middle Bronze IIB (1800–1550)	Old Babylonian Period (1900-1600) ‘Mari Age’ (19th/18th century)
		Hammurabi (1825/1775/1700)
1700		
1600		
.....		
	Late Bronze I (1550–1400)	Kassite Period (1600–1150)
1500		Kingdom of Mitanni (1600–1350)
1400	Late Bronze II (1400–1200)	‘Nuzi Period’ (15th century)
		Middle Assyrian Period (1350–1050)
1300		
		End of Mitanni (ca. 1250)
1200	Iron IA (1200–1150)	
	Iron IB (1150–1000)	
1100		
1000		
.....		

NEAR EAST (3RD/2ND MILLENNIA BCE)

Middle Kingdom (2050–1700)
12th Dynasty

Second Intermediate Period (1750–1550)

15th Dynasty (Hyksos [1650–1550])

Hittite Old Kingdom (1650–1500)

Mursilis I (ca. 1600)

New Kingdom (1550–1050)

18th Dynasty (1550–1300)

Thutmose III (ca. 1450)

Thutmose IV (ca. 1400)

Amarna Age (14th century)

Akhenaten (ca. 1350)

19th Dynasty (1300–1200)

Hittite New Kingdom (1450–1200)

Šuppiluliumas I (ca. 1350–1325)

Ramesses II (ca. 1280–1215)

Merneptah (ca. 1215–1200)

Ramesses III (ca. 1180–1150)

Defeat of Sea Peoples (Philistines) (ca. 1175)

Battle of Qadesh (ca. 1275)

End of Hittite Empire (1200)

2.3.2.5 Ugarit

On this city-state, see §2.1.3.

2.3.3 *Palestine*

Once the biblical text is eliminated as having little to tell us about the second millennium BCE, we are mainly dependent on archaeology (summarized at §2.1.1), but we do have some inscriptions. It is important to bear in mind that throughout the second millennium BCE, Palestine was mostly under the thumb of Egypt. The history of the region is the history of an Egyptian appendage. The Execration Texts (§2.1.2.1) give the names of some cities. There are also a few texts describing individual episodes, such as the capture of Joppa by the commander Djehuty under Thutmose III (Goedicke 1968, though this seems to be legend with a historical core), as well as accounts of military forays into Palestine and Syria by various Egyptian kings (e.g., the taking of Megiddo by Thutmose III [ANET: 234–8] or the campaigns of Seti I into Palestine and Syria [ANET: 254–5]). The story of Sinuhe also offers insights (§2.1.2.3). It is the Amarna Letters, however, that give us a real look through the keyhole into the Palestine of the fourteenth century BCE (§2.1.2.2). The rulers of a number of cities are named and an account (often distorted, of course) of their activities is recorded for the Pharaoh by their neighbours (and rivals). Of particular interest are the kings of Shechem and Jerusalem who seem to dominate the highlands.

CHAPTER 3

Late Bronze to Iron IIA (ca. 1300–900 BCE): From Settlement to Statehood

This chapter covers a long period of about four centuries or more. No doubt many will feel that this is too long a period of time to be covered by one chapter, especially considering the importance of this period for the current debate. Yet this chapter encompasses such a long period of time precisely because interpretations vary so widely. If Iron I had been separated from Iron IIA by being in separate chapters, it would already have sided with one side of the debate against the other. For Finkelstein's Iron I extends through much of the tenth century, contrary to conventional chronology. Thus, it seemed best to treat this whole period together, even if it makes for a rather long chapter.

3.1 Sources

3.1.1 Archaeology

One of the most significant areas of discussion is that relating to chronology, specifically the debate about the 'low chronology'. This is summarized above (§1.2.3.4), with that section presupposed in the discussion here. Because of the debate over chronology, the following archaeological survey will attempt as far as possible to refer to archaeological periods (Iron I, Iron IIA, Iron IIB) rather than specific dates. Note that the LC and MCC coincide from the eighth and seventh centuries onward (Finkelstein 1999a: 39). It is generally agreed that Iron IIB ends, and Iron IIC (or, as some prefer, Iron III) begins, with the fall of Samaria and/or the invasion of Sennacherib (ca. 720/701 BCE; see the charts in Mazar 2005; cf. Ofer 2001: 30–1). The stratigraphic

and chronological framework for the Iron Age in Palestine is based on several key sites: Megiddo, Lachish, Jerusalem and Samaria (Ussishkin forthcoming a). Jerusalem is probably the most contested site of all those in the whole of Palestine. It is vital for the period covering the so-called 'united monarchy'. Finally, as noted above, the invasion of Shoshenq has been used extensively to date specific sites with a destruction layer. Yet whether Shoshenq's topographical list relates an actual invasion of his or is only a traditional compilation is very much a debatable point. In any case, a widespread destruction by Shoshenq is by no means a certainty. Also, as Ussishkin points out, Shoshenq's treatment of various sites may have differed; for example, the stela erected in Megiddo suggests a site occupied rather than destroyed.

The general prosperity of the LB age came to an abrupt end sometime about 1200 BCE with the destruction of the main centres in Palestine: Akko, Hazor, Megiddo, Beth-Shean, Lachish, Ashdod. Yet the 'conventional wisdom' that the Palestinian city-state system came to an end at this time oversimplifies what happened (Finkelstein 2003a: 75–9). Some city-states probably did decline or disappear with the destruction of their urban centres, but the rural sector generally experienced continuity, both demographically and culturally, as is indicated by such sites as Tel Menorah, Tell el-Wawiyat and 'Ein Zippori. The peasants in the vicinity of the ruined LB cities lived as they already had, and the northern valleys remained densely settled. In Iron I (eleventh century, according to Finkelstein) the main centres began to recover, the main exceptions being Lachish and Hazor. This 'new Canaan' was prosperous because of stability in the rural sector and trade with Phoenicia, Cyprus and even further afield.

If we begin with the site farthest north, the current excavators of Hazor have retained Y. Yadin's assignment of stratum X to the tenth century and stratum VIII to the Omride dynasty (Ben-Tor 2000; Mazar forthcoming b). Although some ¹⁴C dates contradict this, Mazar notes that it seems difficult to compress strata XB to VIII into 70 years in the ninth century, as required by the LC (see Finkelstein's response in 2005a: 38). The conventional view equates Megiddo VA–IVB, Hazor X, Gezer VIII and Beersheba V – all seen as evidence for the united monarchy (Finkelstein 1996a: 177). Thus, according to Mazar (forthcoming b), Yadin's thesis of Solomonic architecture at the three sites (Megiddo, Hazor and Gezer) might still be correct. Finkelstein (1999b: 59) argues, however, that Yadin's equation of Hazor X with the time of Solomon creates chaos with strata IX, VIII and VII, leaving no place for the important activities of Hazael in northern Israel. A. Zarzeki-Peleg's study of the pottery assemblages connects Megiddo VIA with Jokneam XVII, while Hazor XB is later (1997: 284). Thus, Finkelstein (1999b: 60) argues, Hazor X was built by the Omrides.

If we move back to LB, A. Ben-Tor (1998) is apparently still of the opinion that the Israelites destroyed Hazor XIII and then settled it in stratum XII (thus continuing to agree – more or less – with Yadin's interpretation). D. Ben-Ami (2001) gives a somewhat different interpretation. He makes several corrections to previous interpretations (2001: 165–70, my formulation). First, the destroyers of LB Hazor (stratum XIII) left the site desolate and uninhabited. However, the 'impressive Late Bronze city of Hazor with its magnificence, monumentality, high status and influence ... underwent a violent destruction that apparently cannot be related to the process of early Israelite settlement in Canaan' (Ben-Ami 2001: 168–9). Second, there was a gap in settlement for a period of time. Third, the settlement of Iron I was a temporary encampment whose presence was indicated by a large number of refuse pits. Fourth, although the identity of those in this encampment is unknown, the material culture is new, with substantial differences that suggest a different population. Fifth, Yadin's identification of two separate settlement strata (XII and XI) is unsupported and only one occupational phase is indicated by the remains.

In the upper Galilee the ceramic continuity from LB II to Iron I indicates that the population was indigenous rather than immigrants as pictured by the biblical text (Josh. 19.24–48; Bloch-Smith and Nakhai 1999: 81). Settlement of the Jezreel and Beth-Shean valleys flourished in LB, with a significant Egyptian presence at the administrative centres of Megiddo and Beth-Shean. The collapse of Egyptian dominance resulted in a general decline, with an impoverished culture (Bloch-Smith and Nakhai 1999: 81–8). A number of small settlements existed in this area, the majority established in the twelfth and eleventh centuries, with a material culture suggesting continuity with LB (Gal 1992: 84, 92). The suggestion that a new population took control (Bloch-Smith and Nakhai 1999: 83) appears to have little or no archaeological support.

Late Bronze II Megiddo (strata VIII/VIIB–VIIA), with an estimated area of 11 hectares, was one of the most prominent cities of Palestine (Finkelstein, Ussishkin and Halpern 2000: 593). With its monumental buildings it appears to have been the city-state centre for the king and his elite supporters. Megiddo thus seems to provide a good example of the nature of the major city-states in Canaan during the later part of LB (over 30 sites were apparently subordinate to Megiddo), though the LB city was apparently unfortified. The absolute dating of the LB II strata is not clear: according to Finkelstein, stratum VIII can hardly post-date the mid-fourteenth century; it is unlikely that there was no palace during the Amarna period. Stratum VIIB alone would represent the city of the thirteenth century. Stratum VIIA was then built in the late thirteenth century and destroyed in the second half of the twelfth. Strata

VIII–VIIA therefore all represent one phase of urban continuity. Ussishkin agrees that Megiddo was continuously settled throughout LB, including the Amarna period, but the character of settlement was not uniform throughout the time. Significantly, most or all the monumental buildings were constructed after the Amarna period, while the Amarna settlement was relatively modest. The conclusion seems to be that the stratum VIII royal palace and gatehouse and some other public buildings were constructed in the thirteenth century.

Megiddo VIIA was destroyed in the second half of the twelfth century; rebuilt as VIB in Iron I it began to prosper again in VIA (Finkelstein, Ussishkin and Halpern 2000: 594–5). This city was destroyed in a major conflagration. Megiddo VB was very different in character, with Iron Age traits in its material culture and layout (Finkelstein, Ussishkin and Halpern 2000: 595–6). In the transition from VIA to VB there is a change both in the pottery tradition and the layout of the city; nevertheless, it remained an imposing administrative centre (Finkelstein, Ussishkin and Halpern 2000: 597). Ussishkin (2007) argues that Megiddo stratum VA–IVB or parts of it preceded the Omri dynasty and that this stratum (or the preceding stratum VB) was probably the city captured by Shoshenq in the late tenth century; however, Shoshenq did not destroy the city but intended to make it his administrative centre. For a discussion of Megiddo in relation to Samaria and Jezreel, see §4.1.1.

Thus, it seems that at least in the northern part of the country, LB Canaan rose again in the late eleventh and early tenth centuries from the blow of the mid-twelfth century (Finkelstein 2003a: 77). Other city-states seem to have emerged at that time, at Tel Kinneret, Tel Rehov, Tel Dor and possibly Tel Keisan. Iron I Kinneret replaced LB Hazor as the centre of the upper Jordan Valley, while Iron I Tel Rehov was probably the focus of a territorial entity that covered the Beth-Shean and eastern Jezreel Valley. Late Bronze Tel Keisan continued to prosper and seems to have served as the central site of the northern coastal plain (including Akko, and Tell Abu Hawam IVA as its port). Iron I Dor dominated the coastal plain of the Carmel Ridge and possibly replaced LB Gath (Jatt) as the main centre of the region.

It has long been suggested that a number of innovations in technology took place in the central highlands: terracing, plastered cisterns, the 'Israelite house', collared-rim jars and iron (Dever 2003a: 113–25). Unfortunately, it seems there is no list of technologies that is exclusive to this region and time or that can serve as 'Israelite ethnic markers' (§1.2.4; Killebrew 2005: 171–81; Bloch-Smith 2003: 406–11; Kletter 2006: 579; Hesse and Wapnish 1997). Terracing was used as early as EB; the early Iron Age settlers in the highlands made use of an existing technology (Gibson 2001). It is now argued that the collared-rim jar was already in use in the thirteenth century along

the Levantine coast and is the product of a particular lifestyle or economy (such as the distribution of rations to employees) and is not exclusive to the Cisjordan highlands or Israel (Killebrew 2001; Raban 2001; Herr 2001). A. Faust and S. Bunimovitz (2003) recently argued that the four-room house was particularly Israelite, though they admit that a few such houses occur elsewhere, including in Transjordan. Iron seems to have been little used, despite the name 'Iron I': weapons and tools were mainly bronze (Bloch-Smith 2003: 417–20).

In this context of unique technologies, the question of pork consumption has also often been discussed, as an ethnic indicator. The subject is a complicated one, though there is a considerable drop in evidence for swine in the highlands during Iron I and Iron II, in contrast to LB and also the coastal plain where pigs remained a part of the diet. But Hesse and Wapnish conclude that 'no human behavioral evidence exists to indicate that pig avoidance was unique to any particular group in the ancient Near East ... Lots of people, for lots of reasons, were not eating pork' (1997: 261). Z. Lederman and S. Bunimovitz (forthcoming) found that the total absence of pig bones in Beth-Shemesh in Iron I was consistent with the minimal percentage of pig bones in the hill country, in contrast to sites identified as Philistine. Beth-Shemesh became a sensitive seismograph for social changes between the regions, one such conspicuous boundary being pork avoidance. This allows that the pig taboo as an ethnic marker might have begun in the Shephelah and extended into the highlands, rather than the other way round.

For the area of Ephraim we have the preliminary survey results (Finkelstein 1988–89: esp. 144–54; Finkelstein 1988: 121–204). The late Bronze Age had seen a demographic decline, but Iron I produced a 'settlement wave of unprecedented intensity', especially in the desert fringe and the northern central range (Finkelstein 1988–89: 146). This produced a pattern of major centres accompanied by a peripheral population. Finkelstein (1988–89: 148) argues that the population was densest in the east at the beginning of Iron I but then moved toward the west, with the cereal/animal husbandry economy also shifting toward a horticultural one; however, Bloch-Smith and Nakhai (1999: 71) assert that the archaeological data do not support either of these conclusions. Yet a comparison of Iron I and Iron II does indicate just such a population shift (Finkelstein 1998b: 357). Regardless, it does appear that the Ephraimite and Manassite regions had the densest population of any region west of the Jordan. Settlement of the less favourable slopes and foothills, with trees and brush to be cleared, came later in Iron I and continued into Iron II. In Iron II almost all the regions of Ephraim were intensively settled (Finkelstein 1988–89: 151–4). Compared with Iron I, the population had shifted west, with some of the large sites in the east abandoned: Shiloh,

‘Ai and Khirbit Raddana. Sufficient grain was grown in some regions, apparently, to allow the western regions to concentrate on the important wine and oil production. In the southern central range of the Ephraimite hills some sites were abandoned and a fall in the population generally occurred in this region.

In LB and early Iron I, a clear difference separated the north and south in the central hill country between the Jezreel and Beersheba valleys (Finkelstein 1998b: 361; 1999a: 43–4). The north experienced significant continuity, with most of the main sites continuing from LB into Iron I. The Negev and Judaeen hills had hardly any sedentary sites in LB; this long settlement gap came to an end in early Iron I. There was a large increase in small sites in early Iron I, especially in the central hill country (Stager 1998c: 134–7; Dever 2003a: 97–100). This increase was largely in the north, especially in the areas of Ephraim and Manasseh, and almost entirely north of Jerusalem. The Judaeen hills had hardly any new sites in the early Iron I, and the region still remained highly pastoral (Finkelstein 1988: 52–3).

With regard to the Judaeen hills, however, we find a significant difference of opinion. Most of the early Iron I sites were between Jerusalem and Hebron, with hardly any south of Hebron. According to A. Ofer the mid-eleventh to the tenth centuries saw a rapid growth in the Judaeen hills, including the more peripheral areas (Ofer 1994: 102–4). Based on his survey results, Ofer (2001; 1994) argued that from the mid-eleventh to the eighth century the population nearly doubled in each century. To Iron IIA (a single identifiable ceramic stage) are to be assigned Tel Qasileh XI–IX, Izbet-Şartah II–I, Beersheba VIII–VI, Tel Edar III–II, Arad XII–XI (Ofer 2001: 30–1). We see the beginning of settlement in the pasture areas, and the first inhabitants of the desert fringe sites (e.g., Tekoah, ha-Qain, Ma‘on, Carmel). The Shephelah only began to be populated at this date.

Some of Ofer’s interpretation has been challenged. G. Lehmann (2003) criticized Ofer’s survey from a methodological point of view (citing a recent publication of A. Faust). He noted that deciding between the conclusions of Ofer and Faust was currently impossible because the political situation prevented the necessary testing. Nevertheless, Lehmann (2003: 133, 141, 157) went on to argue, in contrast to Ofer, for a much smaller settled area and a population more like 10,000 in Iron I and 16,000 in Iron IIA Judah (including Jerusalem), but these are maxima: he reckoned Iron IIA at 5000 to 10,000, with Iron I correspondingly less. The built-up area of the Shephelah was twice as large as Judah in Iron I and perhaps even three times as large in Iron IIA (2003: 134). Yet Z. Herzog and L. Singer-Avitz (2004: 220) critiqued Lehmann for nevertheless accepting too many of Ofer’s data without question, though their study confirmed the view that the highlands

of Judah (including Jerusalem – see below) were relatively sparsely settled, in contrast to the lowlands.

From a broader perspective the population density of the Judaeen hills was significantly less than the areas to the north. Ofer (1994: 107) notes the difference between the results of the Judaeen hills survey and those of the Manasseh hill country and the land of Ephraim surveys. In Iron I Ephraim and Manasseh were four times that of the Judaeen hills, and twice that in Iron IIA. In the northern highlands most of the LB sites continued to be inhabited in Iron I, in contrast to the Judaeen highlands which had almost no LB settlements (Finkelstein 1999a: 43–4). Many highland settlements disappeared in the later Iron I (Bloch-Smith and Nakhai 1999: 78). Growth in the Judaeen hills was slow from Iron I to Iron IIA, in contrast, for example, with Benjamin where the growth was 243 per cent (Lehmann 2003: 134). Penetration into the desert fringe and an increase in the population of the southern regions of the Judaeen hills in general characterized Iron IIA (Ofer 1994: 104). The continuity of settlement from Iron I to Iron II was over 70 per cent, but the villages increased in Iron II, with 60 per cent new (Lehmann 2003: 147–50). Unlike Iron I, the sites were not at the highest elevation but tended to be in the middle slopes, suggesting that the settlers were feeling more secure. The more important settlements lined up with the north–south watershed in both Iron I and IIA. The settlement peak came about in the eighth century (Ofer 1994: 105). This all suggests that developments in the central territory of Judah lagged significantly behind those of the north, which has implications for evaluating state development in general and the united monarchy in particular.

As noted above, Jerusalem is probably the most contested site in Palestine. The main question is what kind of a settlement Jerusalem was in Iron IIA: was it a minor settlement, perhaps a large village or possibly a citadel but not a city, or was it the capital of a flourishing – or at least an emerging – state? Assessments differ considerably, with Ussishkin, Finkelstein, Steiner, Herzog, Lehmann and Singer-Avitz supporting a minimal settlement; but many (including A. Mazar and, especially, Cahill) arguing for the latter. Using Rank Size Theory, Ofer had argued that the graph of settled sites required Jerusalem to be included as the capital for the Judaeen hill country to follow the expected pattern (1994: 97–8, 102–4); however, Lehmann's Rank Size analysis shows just the opposite (2003: 149–51). D. W. Jamieson-Drake (1991) was one of the first who queried the status of Jerusalem, concluding that it did not have the characteristics of a capital city, including monumental architecture, until the eighth century BCE. Jamieson-Drake has been much criticized for gaps in his data, but it seems that he was right as far as he went, though he gave an incomplete description (Steiner 2001: 284).

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J. M. Cahill (2003; 2004) has been one of the most vociferous voices arguing for a substantial city as early as the tenth century. She dates the 'stepped stone structure' to the Late Bronze/early Iron transition, arguing that both the stepped mantle and the terraces below it were built together as a single architectural unit (Cahill 2003: 42). The fortification wall built during the Middle Bronze Age remained standing and was repaired and used until Iron IIB (2003: 71). Cahill summarizes her conclusions for the tenth century:

My own view is that the archaeological evidence demonstrates that during the time of Israel's United Monarchy, Jerusalem was fortified, was served by two complex water-supply systems and was populated by a socially stratified society that constructed at least two new residential quarters – one located inside and the other located outside the city's fortification wall. (Cahill 2004: 20)

Reinforcing Cahill's interpretation, it has recently been argued that a building at the top of the 'stepped stone structure' dates to David's time and could be his palace (E. Mazar 2006), though the proper archaeological details have not been published; however, acceptance of her interpretation is by no means universal, if conversations with other archaeologists are anything to go by. The 'stepped stone structure' is itself the subject of considerable dispute, with several positions being taken (Lehmann 2003: 134–6). In contrast to Cahill, Steiner (1994; 2003b: 351–61; 1998) argues from Kenyon's excavations that the stepped stone structure was quite different in extension and construction method from the terrace system. Where the terraces existed, only a mantle of stones was added, but where there were no terraces the structure was built up from bedrock. The dating and construction is confirmed by the pottery in the fill of the terraces which was Iron I, with no mix of later material. Steiner would date its construction to the tenth or early ninth century. In sum, the terrace and stepped structure systems do not have similar boundaries, identical pottery or the same construction techniques. From Kenyon's excavations she feels there is enough evidence to show that the structure went out of use in the late eighth or early seventh century BCE when a new city wall was built further down the slope.

As described by Steiner (2001; 2003a; 2003b), Jerusalem of the tenth and ninth centuries was a small town occupied mainly by public buildings, not exceeding 12 hectares and with approximately 2000 inhabitants (compare this with Lehmann's 2 hectares and 300–600 inhabitants in Iron I, and 4 hectares and 600–1200 inhabitants in Iron IIA [Lehmann 2003: 135–6]). It exhibits the characteristics of a regional administrative centre or the capital of a small, newly established state, the towns of Megiddo, Hazor, Gezer and

Lachish showing similar characteristics at the same time. Excavations on the Ophel show the earliest buildings there date only from the ninth century. E. Mazar had argued that the fortified complex of this area south of the Temple Mount had been constructed as early as the ninth century, but the more likely date is between the eighth and the early sixth centuries BCE (Killebrew 2003: 336). E. A. Knauf (2000) argues that the centre of the Davidic city has not been found because it would have been the area north of the Ophel, the area of the Temple Mount. Although it is not possible to test this hypothesis now, that section of the hill was a militarily strategic area and would have had to be incorporated into any settlement on the southeastern hill.

The argument that the MB wall was used as a city wall in LB, Iron I, and Iron IIA and IIB has no archaeological support: Jerusalem lacked a fortification wall until the mid-eighth century when the MB IIB wall was partially built over and partially reused for a new fortification wall (Killebrew 2003: 334; Ussishkin 2003: 110–11). The lack of other finds relating to fortification suggests that Jerusalem was unwallled and unfortified between LB and Iron IIB (sixteenth to mid-eighth centuries), and thus Jerusalem was ‘at best modest’ (Killebrew 2003: 334). Also, the elaborate water system of MB IIB went out of use until the eighth/seventh centuries (Warren’s shaft never served as a water system), as shown by the excavations of Reich and Shukron (Killebrew 2003: 334–5).

In the transition from LB to Iron I the Shephelah suffered a massive demographic decline, with 67 sites in LB but 25 in Iron I, and a reduction of settled area from 105 hectares to 60 hectares (Finkelstein 1999a: 44). The Shephelah sites seem to have prospered in the later Iron I (Bloch-Smith and Nakhai 1999: 102–3). A number of lowland features of Iron I later became characteristic of highland Iron II, primarily the collared-rim storejars, the four-room house and the bench tombs. This indicates a movement of some lowland peoples to the east and north (Bloch-Smith and Nakhai 1999: 103). Y. Dagan (2004: 2680–1) concluded that in Iron IIA the sites developed slowly as the process of Judaeon settlement began, with many ‘dispersed’ and ‘isolated’ structures, a situation unknown in earlier periods, indicating a period of stability. With regard to the central site of Lachish, it was rebuilt after a long habitation gap in Iron I (Ussishkin [ed.] 2004: I, 76–87). Although little remains of this city (level V), the inhabitants seem to be a new people with a new material culture. Some have wanted to date level V to the tenth century and connect it with the invasion of Shoshenq, but the latter’s inscription does not mention Lachish (nor any place in Judah proper except Arad), nor has any destruction layer been uncovered. Based on A. Zimhoni’s study of the pottery (2004: IV, 1707) Ussishkin dated level IV to the mid-ninth century BCE and level V to the first half of the ninth. (This

incidentally supports the LC, but Ussishkin notes that the interpretation is not conclusive.) If Lachish V dates to the ninth, Rehoboam could not have fortified the site (2 Chron. 11.5-12, 23) nor would Shoshenq have destroyed it.

In the Negev there was a settlement gap throughout LB but a renewal in population came in Iron I. The Beersheba valley is normally too arid for farming, but there have been a few periods of sufficient rainfall, including the period from the thirteenth to the tenth centuries (Herzog 1994: 125–6). This increased fertility and the peaceful conditions (apparently), and led to population growth (approx. 1000 in the eleventh century). New settlements included Tel Masos, Arad, Tel Haror, Tel Sera', Tel Esdar, Tel Beersheba, Nahal Yattir and other sites. Some sites (e.g., Tel Sera' and Tel Haror) were settled by Philistines, but the identity of those moving into other sites is controversial (see the survey in Herzog 1994: 146–8). The biblical text mentions a number of groups associated with this region: Judah and related groups, Simeon, Amalekites, Canaanites, Kenites, Calebites and so on.

The transition from Iron I to Iron IIA was marked by a significant abandonment of settlements. Tel Masos was destroyed or abandoned, but it was rebuilt as a small fortress, and a number of other fortresses were also established. It has been argued that these changes were the result of intervention by the Jerusalem monarchy, but 'the spatial distribution of the sites in the valley and the demographic estimates does not support this model'; rather, a good case can be made that the cause was environmental (Herzog 1994: 143–5). Other suggestions are expanding highland settlements or Shoshenq's invasion (Finkelstein 2003a: 78–9). Certainly, the sites most uncontroversially associated with the campaign of Shoshenq are found in the Negev. The 'Greater Arad' of Shoshenq is generally agreed to be Tel Arad (Finkelstein 1999a: 38–9). Arad has been much debated over the years, with later studies substantially disagreeing with the original excavator's. Stratum XI had been identified with Shoshenq's invasion, but Zimhoni (1985) and A. Mazar and E. Netzer (1986) concluded that XI must be later than the tenth century, which would make stratum XII the Shoshenq level. The latest excavator Z. Herzog (2001; 2002: 58–72) agrees with this. Thus Arad XI and Beersheba V (with the first Iron Age fortifications in Judah) must be in the ninth century.

3.1.1.1 Analysis

There is wide agreement that Arad XII and related Negev sites are to be related to Shoshenq's invasion (Mazar forthcoming a; forthcoming b). The

use of Shoshenq's list of cities provided a chronological anchor of crucial importance (Ussishkin forthcoming a). Albright's assumption that Shoshenq destroyed the whole country has been widely accepted, but his actions might have been varied: the erection of a royal stela at Megiddo shows that Shoshenq aimed to hold the city, not destroy it. Thus, his list is useless as a secure archaeological and chronological anchor, the only possible exception being Arad XII. If Arad XII dates to the second half of the tenth century and not later, this affects the dating of Lachish V, Tel Beit Mirsim B and Tel Beersheba VII; however, Ussishkin (forthcoming a) has some doubts about the reliability even of this synchronization.

The main advantage of the 'low chronology' is that it closes the 'dark age' of the ninth century (Finkelstein 1996a: 184–5). Its main disadvantage is that it changes the entire understanding of the emergence of the Israelite state. The monuments previously associated with the united monarchy are redated from the second half of the tenth century to the early ninth. The 'low chronology' forces reconsideration of several issues relating to the archaeology of proto-Israel. In the hill country, settlements of the tenth century are not much different from those of the eleventh; thus, the real transformation came about 900 BCE rather than about 1000 BCE, which has consequences for a united monarchy. In the northern highlands this transformation brought significant growth in the number and size of sites and expansion into new frontiers and niches, while the southern highlands were only sparsely settled in early Iron II.

'Accepting the Low Chronology means stripping the United Monarchy of monumental buildings' (Finkelstein 1996a: 185). According to Finkelstein (1999b: 39) many of the strata dated to the eleventh century should now be the tenth (Megiddo VIA; Beth-Shean Upper VI, Tel Hadar IV, Jokneam XVII, Beersheba VII, Arad XII). Important for our purposes are those redated to the ninth century according to the LC: Megiddo VA–IVB, Beersheba V, Arad XI. Those strata dated to the eighth and seventh centuries are not affected. The main mounds in both the north and the south (Megiddo, Gezer, Beersheba, Lachish) would be dated to the ninth century BCE or even later. Although inscribed seals and impressions are known from the mid-ninth century, they are mainly found from the eighth century onwards. Finally, the LC closes the unexplained gap between the monumental architecture traditionally assigned to the tenth century and the evidence of public administration for which we have clues in the late ninth to the eighth. In sum, the tenth century is closer to the previous period than to Iron II; the real revolution came in the ninth century, more in the north than the south. The line between Iron I and Iron II came in the early ninth century rather than about 1000 BCE. The kingdom of David and Solomon would have been a chiefdom or early state

but without monumental construction or advanced administration (cf. the early Ottoman Turks or Shechem in the Amarna age).

Mazar's MCC seems to have been fairly widely accepted, but its extended Iron IIA spanning both the tenth and ninth centuries means that 'it would make the position of those who wish to utilize archaeology for secure historical interpretation of the 10th-9th centuries BCE harder to sustain' (Mazar forthcoming a; cf. forthcoming b). It also means that events that were dated to the tenth century in conventional chronology – and which the LC dates to the ninth century – are left unspecific in the MCC. Ussishkin has expressed his pessimism about resolving the issues about chronology by normal archaeological methods, referring to:

[The] ambiguity of clear stratigraphic evidence in many sites, and the difficulty of comparing pottery from different regions of the country. Hence there are possibilities for different interpretations and different chronologies. In my view, as long as no new additional data are available it would be impossible to solve the chronological differences being debated at present. (Ussishkin forthcoming b)

He goes on to refer to radiocarbon dating, a 'method that has been enthusiastically adopted' by some scholars on both sides of the debate, but 'this method is far from providing conclusive and perfect results', for 'the interpretation of the same ¹⁴C tests can be fitted to different ideologies' (Ussishkin forthcoming a). As quoted above, Mazar has noted the problems with radiocarbon dating but, nevertheless, refers to the results that he feels support his case. It may be that radiocarbon data will lead to a resolution or partial resolution of the debate, but this is likely to be only after a substantial database has been established (Sharon *et al.* 2005).

Another conclusion (only partially divorced from the chronology debate) is the difference between the development in northern Palestine as contrasted with southern Palestine. This was largely due to geographical factors: topography, geology, soil, rainfall, climate in general. The northern region, both the hill country and the lowlands, was much more suited to settlement, farming and fertility in general. The region of Judah was less fertile, had a lower density of population, and developed economically more slowly than Samaria. When one considers the *longue durée*, it would have been extraordinary for the Judaeans highlands to dominate the north in Iron I or IIA. A number of archaeologists argue that the archaeology does not support the text which depicts a Judaeans-highland-centred united monarchy. Those who do argue for archaeological support for the united monarchy generally do so by explicit – or implicit – appeal to the biblical text as the guide for interpreting the archaeology.

Jerusalem remains an area of considerable controversy, but those who maintain that Jerusalem did not develop into a substantial city until Iron IIB have current archaeology on their side (though the building recently found by E. Mazar has intriguing possibilities). Those who maintain an earlier development must argue on the basis of what is presumed to have disappeared or what might be found in the future. This is why a substantial argument is now made that the northern kingdom (in the form of the Omride dynasty) was the prior development to a state in the ninth century, with Judah coming along more slowly, reaching its height only in the eighth century. But the debate continues.

3.1.2 Merneptah Stela

An inscription in the name of Merneptah, dating to his fifth year (conventionally dated to 1207 BCE, though Kitchen insists it is 1209/1208), has the first reference to Israel known and the only reference until the ninth century. Accompanying it are reliefs from the Karnak temple that have been associated with some of the events described in the poem (on these, see below). Most of the inscription is about Merneptah's defeating the Libyans who attempted to conquer Egypt, along with help from a revolt in Nubia. It is only right at the end that statements are made about other peoples supposedly conquered by Merneptah, one of which seems to be Israel:

Tjehenu is seized, Khatte is pacified,
Pekana'an (Gaza) is plundered most grievously
Ashkelon is brought in and Gezer captured,
Yeno'am is turned into something annihilated,
Israel is stripped bare, wholly lacking seed!

[*Ysr3r fk(w) bn prt.f*]

Kharu has become a widow for Egypt
And all lands are together at peace.

(translation of Redford [1986b: 197]; textual quote from Niccacci 1997: 64)

Although the reading 'Israel' (for Egyptian *Ysr3r*) has been widely accepted, not everyone agrees. For example, the name *Ysr3r* has been read as 'Jezreel', as well as some less credible renderings (Eissfeldt 1965; Margalith 1990: 228–30; Hasel 1998: 195–8; Hjelm and Thompson 2002: 13–16). From a philological point of view, this seems an unlikely reading, as do some of the other suggestions (Hasel 1998: 197–8; Kitchen 2004: 270–1). All in all, it seems that the reference to Israel is reasonably secure. Much debate has

centred around the determinative (cf. Yurco 1986: 190 n. 3; Hasel 1998: 198–9). The other three names have the three-hills and throw-stick signs, which are normally used for a foreign territory, whereas Israel has a seated man and woman with the throw-stick, which suggests a people rather than a fixed geographical site. These data have been used in arguments about Israel's origins (§3.2.4.2). Another question concerns the phrase 'his seed is not' (*bn prt.f*). It has often been taken metaphorically to refer to 'descendants, offspring' (e.g., Niccacci 1997: 92–3), but recently it has been argued that this means 'grain', suggesting that Israel is a sedentary community of agriculturists at this time (Hasel 1998: 201–3). Rainey (2001) argues strongly that it should be understood as 'descendants', though this translation is taken as evidence for his own interpretation of how Israel originated.

The question is, is this inscription only a piece of royal propaganda – a triumph-hymn – with little or no historical value (cf. Hjelm and Thompson 2002)? It is one of four sorts of royal inscription and includes extravagant praise of the king as a matter of course, but this by itself does not resolve the matter because factual material is also included at relevant points in such inscriptions (Kitchen 2004: 260–5). The argument – really, more of an assertion – that Israel is only an eponym ('analogous to Genesis' Israel: the patriarch of all Palestine's peoples') ignores the determinative, which is plural and which refers to a people. According to Kitchen (2004: 271) the oft-made statement that a number of errors in determinatives are found in the inscription is incorrect. As for Israel's being paired with Kharu, this is only one possible analysis. In fact, a number of different literary structures have been seen in the passage (summarized in Hasel 1998: 257–71). There is also the question of whether *Pekana'an* refers to 'Canaan' or 'Gaza'. In spite of Hjelm and Thompson, the conclusion that this inscription 'has been considered correctly as concrete proof of an Israel in Palestine around 1200 BCE' (Lemche 1998a: 75) remains the most reasonable one.

More controversial are the reliefs (Redford 1986b; Yurco 1986; 1997: 28–42; Hasel 1998: 199–201). The reliefs in question give ten different scenes: the first four are the main ones, which picture the Pharaoh triumphant in battle; the fifth pictures bound Shasu prisoners; and the sixth shows Canaanite captives being led to a chariot. Redford (1986b) has argued that the inscriptions originally related to Rameses II and have been altered to fit later rulers, and there is no reason to associate all of them with Merneptah who was in poor health and decrepit when he came to the throne. There 'is absolutely no evidence that Merneptah attacked all these places during his short reign. To the best of our knowledge, during his rule there occurred no triumph over Khatte ... nor any defeat of Gaza or Yeno'am' (Redford 1986b: 197).

F. Yurco has argued (against Redford) that the first four reliefs can be equated with the four names in the inscription (Yurco 1986; 1997: 28–42). In other words, scene 1 describes the conquest of Ashkelon; scene 2, of Gezer; scene 3, of Yano'am; and scene 4, of Israel. He concludes that the scenes pictured agree with the determinative that accompanies each name, with the first three shown as cities and the fourth (Israel) as a people but no city. Yurco's argument that the reliefs are to be ascribed to Merneptah seems to have won over some (cf. Kitchen 2004: 268–70) – though Redford seems to maintain his position (1992a: 275 n. 85). But the equation of the reliefs with the four names in the inscription is rather less secure (Rainey 2001: 68–74). In only the first scene is the site named (Ashkelon), but no names are found in scenes 2–4. Also, it may be that there were once other scenes on the wall that are now missing because of deterioration of the structure. Thus, the relating of specific names to specific scenes is much more hypothetical than Yurco seems to allow.

The interpretation of the reliefs is important to the various antagonists primarily because of the identity of the peoples being defeated. Yurco's main concern seems to focus on the dress of those fighting the Egyptians. He argues that the defeated Israelites have the typical dress of the Canaanites, providing evidence that Israel was like (and thus arose from) the Canaanites. Rainey (2001: 72–4) argues that, on the contrary, the Israelites are to be identified with another group who are pictured in scene 5: the Shasu (also Redford 1986b: 199–200). Although this seems to be a definite possibility, his arguments for a positive identification seem to be no stronger than those of Yurco for the Canaanites.

Finally, there is the question of where Israel is supposed to reside. A number have asserted that it refers to the hill country (e.g., Dever 2001: 118–19). Kitchen (2004) argues that each name refers to a section of Palestine: Ashkelon to the coast, Gezer to the inland area, Yano'am to the Galilee; therefore, Israel would refer to the hill country. This is far from cogent. There is nothing in the inscription to suggest that the individual names were meant to refer to a specific part of the country – Merneptah may just be listing sites and peoples conquered. Also, the sections of Palestine listed for the first three names by Kitchen do not cover all the territory except the hill country: what of the valley of Jezreel, the Jordan Valley, the Negev, the Transjordanian region, the plain of Sharon, and so on? N. Na'aman (1994c: 247–9) points out that it is possible that the author mentioned the cities first and then the people, so there was no sequential listing. The conjectured location is highly speculative: some put 'Israel' in the area of Shechem, but the Egyptians called it 'the land of Shechem' or 'the mountain of Shechem'; putting Israel in Manasseh is nothing more than guesswork. In conclusion,

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it is 'best to refrain from building on this isolated reference any hypothesis concerning the location and formulation of Israel at that time' (Na'aman 1994: 249). Thus, no argument has so far been presented to pin down the exact location in the land of this entity Israel. Ultimately, the only thing we can say is that the inscription proves there was an entity called 'Israel' in Palestine about 1200 BCE. This is an important datum, but it does not allow us to be certain of where it was located (if indeed there was a single location) or the precise organization or status of this entity 'Israel'.

3.1.3 *Medinet Habu and Related Inscriptions*

Rameses III's temple at Medinet Habu is very important for reliefs and inscriptions that relate to the invasion and defeat of the Sea Peoples (*ANET* 262–6; *ARE* IV, §§59–82; O'Connor 2000; Wachsmann 2000), along with a few other sources of lesser importance (Redford 2000: 8). These are not straightforward historical accounts but require a fully critical approach; nevertheless, with a careful reading much can be learned (Redford 2000; see also §3.2.2 below). The following excerpt is taken from *ANET* (italics and square brackets in the original):

(16) ... The foreign countries made a *conspiracy* in their islands. All at once the lands were removed and scattered in the fray. No land could stand before their arms, from Hatti, Kode, Carchemish, Arzawa, and Alashiya on, being cut off at [*one time*]. A camp [was set up] in one place in Amor. They desolated its people, and its land was like that which has never come into being. They were coming forward toward Egypt, while the flame was prepared before them. Their confederation was the Philistines, Tjeker, Shekelesh, Denye(n), and Weshesh, lands united. They laid their hands upon the lands as far as the circuit of the earth ... I organized my frontier in Djahi, prepared before them: – princes, commanders of garrisons, (20) and *maryamu*. I have the river-mouths prepared like a strong wall, with warships, galleys and coasters. (*ANET* 262)

For further on the interpretation of this text, see §3.2.2 below.

3.1.4 *Report of Wenamun*

One interesting text is the *Report of Wenamun*, about a journey to the Phoenician and Palestinian area (*AEL* II, 224–30). The text is usually dated to the early eleventh century BCE, allegedly before Egypt lost its hold on the

region. It appears to give useful insights into the relationship between Egypt and the Phoenician area and contains many useful incidental details.

3.1.5 Shoshenq I's Palestinian Inscription

According to 1 Kgs 14.25-28 a King Shishak of Egypt came up against Jerusalem in Rehoboam's fifth year and took all the treasures of the temple. When an inscription of Shoshenq I (c. 946/945 – 925/924 BCE), founder of the twenty-second Dynasty was found at Karnak listing many topographical sites in Palestine, a connection was made with the passage in the Bible and has been the standard view ever since. All seem to agree that Shoshenq's expedition was a signal event in Israel's history, but precisely what happened on the ground and even when the invasion took place is considerably disputed. The conventional view is heavily informed by the Bible. According to it, Shoshenq's army made a number of destructive raids on various parts of Palestine, destroying many sites in the Negev and even as far north as Megiddo; however, Jerusalem did not fall because the Pharaoh was bought off by Rehoboam.

A number of studies have addressed the issue of Israel/Judah and Shoshenq's 'invasion' beginning with M. Noth's study in 1938 (for a survey of earlier studies, see Wilson 2005; also Schipper 1999: 119–32; Ash 1999: 50–6). Most have assumed that Shoshenq conducted an invasion of Palestine, that the inscription gives some sort of invasion route, that the inscription can be reconciled with the biblical text, and that the archaeology matches the inscription. There have, nevertheless, been some problems, especially the fact that Israel and Judah are not mentioned specifically, that no site in Judah occurs in the inscription, that the toponyms cannot be worked into any sort of itinerary sequence, and that the biblical text says nothing about an invasion of the northern kingdom.

It might not be surprising if a Jerusalem scribe did not record the details of Shoshenq's raids on Israel, but why omit the destructive attacks on the Negev, which was a part of Judah – at least, in the eyes of the Bible? Leaving aside the Jerusalem question, there is still considerable disagreement about how to interpret Shoshenq's inscription. Was it a raid or primarily an occupation – albeit temporary – of the land? Various explanations have been given of the order of toponyms as they might relate to the progress of the invasion, but none has been completely convincing. Now, however, K. Wilson has investigated Shoshenq's inscription in the context of other Egyptian triumphal inscriptions. He concludes:

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- Triumphal inscriptions were designed to extol the Pharaoh's exploits, not provide historical data.
- The reliefs glorify all the exploits of the king rather than a particular campaign.
- The topographical lists are not laid out according to any system that allows a reconstruction of the military route.
- The sites listed may in some cases be those attacked, but others not attacked – indeed, friendly towns and allies – might be listed as well.
- The lists were apparently drawn in part from military records and onomastical lists, which means that some data of value for certain purposes may be included.

The implications of these conclusions are considerable. Rather than recording a particular campaign into Palestine, Shoshenq's inscription may include more than one (as maintained by Knauf forthcoming). This would help to explain the vague nature of the inscriptions that accompany the topographical lists, without clarifying the reasons for or objectives of the 'invasion'. In any case, the precise nature and progress of the campaign(s) cannot be worked out. More puzzling is the lack of any reference to Judah or Jerusalem as such. The argument is that this was in a section of the inscription that is no longer readable. This argument is still maintained by the latest study of the Shoshenq inscription by Kevin Wilson (2005). It must be said that this argument, while possible, is not compelling. Another obvious interpretation is that Shoshenq bypassed Judah – or at least, the Judaeen highlands – because it did not suit his purpose, and the biblical writer got it wrong. Interestingly, the solution that seems to be agreed on by both A. Mazar and I. Finkelstein (both in Grabbe [ed.] forthcoming) is that Shoshenq was indeed interested in Judah because of the copper trade. This could make Jerusalem not just a stage in the invasion but its main object (though not Finkelstein's view). This is an interesting interpretation, though one might ask why Shoshenq then pushed on north as far as the Jezreel Valley if he had already reached his objective.

Finkelstein, among others, argues that the main phase of prosperity was post-Shoshenq and that the sites in the south were primarily not destroyed but abandoned. They point out that Shoshenq also does not mention the Philistine cities, which could be significant. He interprets this as evidence for their control of the copper trade. But whether or not that is right, we have to ask why the Philistines were omitted. If the Egyptian expedition was a general attack on Palestinian cities in Israel and Judah, why should the Philistine plain be left out? Could these cities have a particular relationship with Egypt? Or was the Shoshenq operation a more complex one? David

Ussishkin (forthcoming b) makes the reasonable argument that Shoshenq would hardly set up his stela in a ruined city, but suggests that Megiddo was not just attacked but was occupied to become a regional headquarters. To me, this calls for a rethink of how destructive Shoshenq's raid was, as opposed to dominance and intimidation.

3.1.6 Biblical Text

Most of the texts of relevance to this time period are from the Pentateuch or the Deuteronomistic History (DtrH). Both of these are collections that scholarly consensus regards as late compilations but possibly having early material in certain sections. A detailed analysis is not possible here, but some of the main issues relating to historicity will be touched on.

3.1.6.1 Pentateuch

The old consensus that had developed around the Documentary Hypothesis has gone, though there is nothing to take its place (Rendtorff 1997; Whybray 1987). Some still accept the Documentary Hypothesis in much its original form, but many accept only aspects of it or at least put a question mark by it. There has been much debate around J, the Yahwistic source (Rendtorff 1997: 53–5), and around P, the priestly source (Grabbe 1997b). It seems clear that the Pentateuch was put together in the Persian period (Grabbe 2004a: 331–43; 2006c), though many traditions in it are accepted as pre-exilic: many scholars, for example, date Deuteronomy to the seventh century BCE. The main texts relevant for Israel's settlement in the land are the books of Exodus and Numbers. These revolve around the exodus and are discussed below (§3.2.1).

3.1.6.2 Deuteronomistic History (DtrH)

The composition of DtrH continues to be debated (Römer 2005; Lipschits 2005: 272–304; O'Brien 1989; Campbell and O'Brien 2000). Most specialists choose between an ascription to either a twofold composition, the first edition in the late seventh century and the second in the exilic period, or a single composition in the sixth century. In each case, the compiler(s) would have used a variety of traditions, as well as making their own edits and additions. On the question of Joshua and history, see N. Na'aman (1994c).

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A survey of recent scholarship on Judges is given by K. M. Craig (2003). The historicity of Kings has also been much discussed, recently summarized by Avioz (2005). I have argued that the DtrH used a Judahite chronicle for information on the Judahite and Israelite kings from the early ninth century (§4.1.7.2), but this generally does not help us in this chapter.

3.2 Analysis

Several controversial issues need to be discussed at some length in this section, relating to the history of biblical scholarship (e.g., the exodus and conquest) as well as to current issues. Some of these may no longer be live issues in the minds of many but are still discussed and still accepted by some.

3.2.1 *The Question of the Exodus*

The idea that the ancestors of Israel were in Egypt for a period, that they were oppressed, that they came out of Egypt ‘with a high hand’ (Exod. 14.8), and that they entered the promised land after a period in the wilderness is a major concept in the biblical text. How historical is this? The last part of the picture – the conquest of the land of Canaan – will be discussed below, but here we focus on the general question of the exodus. Several issues are involved:

1. The exodus tradition in the biblical text. The vast bulk of the Pentateuchal text describing the exodus and related events seems to be quite late (Albertz 1994: 23–4, 42–5: ‘exilic or early post-exilic’). The question is whether the exodus is presupposed in early texts. It was once widely argued that the exodus was embodied in certain passages quoting an early Israelite ‘credo’ (Von Rad 1965), but subsequent study suggested that some of these passages (e.g., Deut. 6.21–23; 26.5–9; Josh. 24.2–13) were actually late (Nicholson 1973: esp. 20–7). Some point out that Hosea (12.1; 13.4), for example, presupposes the exodus tradition. Not everyone is confident any longer in such literary analysis; in any case, this would take us back only to the eighth century, long after the alleged event. Whatever the reality, it is clothed in a thick layer of mythical interpretation. The Pharaoh is a generic figure, without a name. A series of ten miracles is enacted – and attempts to find naturalistic explanations (e.g., Hort 1957) miss the point: the aim of the narrative is to magnify the power of Yhwh and his servant Moses.

According to the plain statement of the text, 600,000 men of military age came out; with the elderly, women and children, the number would have been at least three or four million (Grabbe 2000c). The crossing of the Red Sea seems to mix a more naturalistic account, in which an east wind moves the waters (Exod. 14.21), with a more miraculous one in which the sea divides and the water stands on either side like walls (Exod. 14.22-29). F. M. Cross (1973: 121–44) attempted to argue that a naturalistic account, in which the Egyptians died in a storm as they pursued the Israelites across the sea in boats, is reflected in Exod. 15.7, but his philological analysis is flawed (Grabbe 1993b). Thus, even if Exodus 15 is an example of early poetry as some argue (e.g., Robertson 1972; Cross and Freedman 1955; Cross 1973: 121–44), it does not appear to give a picture different from the surrounding narrative (for the argument that Exodus 15 is not early, see Noth 1962). The biblical text does not provide any particular time for Israel's coming out of Egypt, and a number of the dates assigned to the event depend on data not really relating to the exodus itself (e.g., the settlement, the Merneptah Stela).

2. Semites in Egypt. A number of Egyptian texts from the second millennium BCE mention peoples who were non-Egyptian and probably Semitic (see the survey in Malamat 1997). The Egyptian texts refer to 'foreigners' under the categories of 'Asiatics' ('*3mw*'), Nubians and Libyans (Leahy 2001: 548). None of the 'Asiatics' mentioned in Egyptian texts is referred to in such a way as to make one identify them with Israelites (though see the discussion on the Shasu at §2.2.1.4). What it does indicate is that the idea of people from Syro-Palestine – including possibly some early Israelites – living for a time in Egypt is not in and of itself problematic.
3. The Merneptah Stela (see §3.1.2 for a full discussion). Appeals to this text as evidence for the exodus are very problematic. The inscription provides no evidence for any sojourn in Egypt for those identified in the text as 'Israel'; on the contrary, this entity appears to be in Palestine.
4. References in Egyptian texts (Frerichs and Lesko 1997; Davies 2004). There is nothing in Egyptian texts that could be related to the story in the book of Exodus. It is not just a question of the official ignoring of defeats of the Pharaoh and his army. There is no period in the second half of the second millennium BCE when Egypt was subject to a series of plagues, death of children, physical disruption of the country and the loss of huge numbers of its inhabitants. Occasionally, a scholar has seen a remarkable resemblance between Moses and an Egyptian official, but the arguments have not met widespread acceptance. At most, one could

say that a memory of the Egyptian figure was used to create the figure of Moses in the biblical text. G. I. Davies (2004) surveys almost all the evidence available, but little of it is very compelling; indeed, he finds the attempts to equate the exodus tradition with certain figures known from Egyptian sources as 'not very convincing'. Thus, his conclusion that the exodus 'tradition is a priori unlikely to have been invented' appears tacked on rather than arising from his data.

5. Egyptological elements in the exodus narrative. Some have argued that elements within the text fit the period of Rameses II (Hoffmeier 1997), but this is not sufficient; one must show that they do not fit any other period in history (see nos. 6–7 below). It has been widely accepted that there are names and other references that suggest some knowledge of Egypt in the exodus narrative, but how early are they? What is notable is that there are few incidental or accidental references to Egypt, such as one might expect, unlike some other biblical passages such as found in Isaiah and Jeremiah; most of what is present is topographical (Redford 1987: 138). More important, a number of the Egyptian elements in the exodus story are anachronistic, for example, reference to the 'land of Goshen' which occurs only in later Egyptian texts (Redford 1987: 138–49). There is no agreement among Egyptologists about elements that could only be dated to an early period.
6. The cities Rameses (Pi-Ramesses) and Pithom (Exod. 1.11). The first problem is identification of the sites in question. Pi-Ramesses is widely identified with Qantir (Pusch 2001; Bietak 1987: 164; Hoffmeier 1997: 117), though Redford asked where the 'Pi' (Egyptian *pr* 'house') of Pi-Ramesses had gone (1963: 408–10; 1987: 138–9; but cf. Helck 1965). No agreement about the identity of Pithom (Egyptian *pr-Itm* 'the house of Atum') has been reached (Wei 1992). Many argue for Tell el-Maskhuta (Redford 1963: 403–8; Holladay 2001), but this site was not settled between the sixteenth and the seventh centuries. The nearby site of Tell el-Ratabah is another possibility, but it was reoccupied only about 1200 BCE (Wei 1992; Dever 1997a: 70–1). It is therefore difficult to understand Davies' statement that 'they are more likely as a pair to belong to a tradition that originated in the Ramesside period than to a later time' (2004: 30). If Tell el-Maskhuta was known as 'Pithom' from about 600 BCE (Davies 2004: 30) and topographical names with 'Rameses' were also widespread in the first millennium BCE (Redford 1987: 139), this argues that the tradition of Exod. 1.11 was likely to be late, rather than Ramesside.
7. The supposed route of the exodus from Egypt. Some have argued that the route of the Israelites' journeyings in the Bible matches the actual

topography and Egyptian settlements on the ground (Hoffmeier 2005; Krahmalkov 1994). A more careful look shows, however, that the text does not reflect the fifteenth or thirteenth centuries BCE but the seventh or eighth (MacDonald 2000: 63–100; Dever 2003a: 18–20). Some of the itineraries are rather vague, showing little actual knowledge of the topography supposedly being described (Deut. 2.1-25; Num. 21.10-20), with Num. 33.1-49 going the farthest in suggesting knowledge of a real travel route (MacDonald 2000: 98). Only at the end of Iron II (but not Iron I or early Iron II) were most of the sites that can be identified actually occupied. Most scholars argue that the itineraries in Exodus and in Numbers 33 are the result of late editing of several different traditions that do not presuppose the same route (Haran 1976; Davies 1990).

8. Conquest of the Transjordanian region. Trying to extract historical data from this tradition is difficult. There is some indication, for example, that at least some of the peoples listed in the tradition are simply creations from the names of mythical figures. Some of the most feared inhabitants of the land are the Anakim who are descended from the Rephaim (Num. 13.35; cf. Deut 1.28; 2.10, 11, 21; 9.2; Josh. 11.21-22; 14.12, 15; 15.14). One of the main figures is Og of Bashan. He is said to be from the remnant of the Rephaim and dwells in Ashtarot and Edrei (Num. 21.33-35; Deut. 1.4; 3.10-11; Josh. 9.10; 12.4; 13.12; cf. Num. 13.33). These names are significant. Other passages (such as Job 26.5; Ps. 88.11-13; Isa. 26.14, 19; Prov. 9.18), as well as the Ugaritic texts, associate the Rephaim with the dead. The god Rapha'u of an Ugaritic incantation seems to dwell in Ashtarot and Edrei (*KTU* 1.108). Thus, it appears that myth has been historicized, and the shades of the dead have been turned into ethnographical entities. The writer seems at times to have taken traditional or mythical names and used them to create a narrative about ethnic groups. There is also some evidence that the writer has drawn on topographical knowledge of the eighth or seventh centuries to draw up his list of journeys.
9. Archaeological evidence. No event of the size and extent of the exodus could have failed to leave significant archaeological remains. Israel's itinerary has already been discussed (point #7 above). According to the book of Numbers (10.11; 12.16; 13.26; 20.1, 22; 33.36) much of the 40 years of 'wandering' was spent near Qadesh-Barnea. This and related sites in Sinai and southern Palestine should yield ample evidence of a large population in this region. Yet we find nothing (Finkelstein and Silberman 2001: 62–4). Qadesh (Tell el-Qudeirat) itself has been extensively excavated but shows no habitation between the Middle

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Bronze Age and the tenth century BCE (Cohen 1981; 1997; Dever 2003a: 19–20) or even later (Ussishkin 1995a).

Opinions about the historicity of the exodus are divided. Despite the efforts of some fundamentalist arguments, there is no way to salvage the biblical text as a description of a historical event. A large population of Israelites, living in their own section of the country, did not march out of an Egypt devastated by various plagues and despoiled of its wealth and spend 40 years in the wilderness before conquering the Canaanites. This does not rule out the possibility that the text contains a distant – and distorted – memory of an actual event. Some feel that the tradition is so strong in the Bible that some actual event must lie behind it, though it might be only a small group of (slave?) escapees fleeing Egypt (a view long and widely held). This is accepted even by some of those proposing theories about the indigenous origin of Israel in Canaan (see below, §3.2.4.2). Some think it might even be a hazy remembrance of the Hyksos expulsion from Egypt in the sixteenth century BCE (e.g., the Egyptologist A. Gardiner [1933]).

Yet others point out that there is no necessity for assuming there was an exodus in the early history of Israel (Dever 1997a: 7–21). There is no external evidence for such an event, and any arguments must depend on the biblical tradition; however, since we know of many Egyptian connections with Israel and Judah at later times, from the time of the monarchy to the Persian and Hellenistic periods (cf. Isa. 19.19–25; Jeremiah 42–44), this could have been sufficient to give rise to the story in the biblical text. Many scholars now agree that there is little clear evidence that the biblical tradition is an early one.

3.2.2 *The Sea Peoples and the Philistines*

According to the biblical text the Philistines were the main enemy of Israel through much of its early history. This picture is manifestly wrong in certain parts; for example, the Philistines have been anachronistically projected back as settled in the land in the early second millennium BCE (Gen. 21.22–34; 26.1–18; Finkelstein [2002b: 152–4] suggests that this reflects an eighth or seventh century context). Yet other parts of the biblical picture may be correct; we can know only after looking at it carefully. We know about the Sea Peoples from a number of sources (see the survey in Noort 1994; also Oren [ed.] 2000; Ehrlich 1996: 1–21). The main sources for the Philistines are the Egyptian texts, primarily the Medinet Habu reliefs and inscriptions (§3.1.3), and the material culture. About these, however, there is considerable debate.

As C. Ehrlich (1996: 9–13) indicates, there are two main interpretations (depending heavily on the Medinet Habu inscriptions and other documents but also bringing archaeological data into the question). The ‘maximalist’ interpretation argues that Rameses III fought a coalition of Sea Peoples (Peleset [Philistines], Sherden, Tjeker, Shekelesh, Denyen, Tresh and Weshesh) in both sea and land battles, defeated them and forced (or allowed) them to settle in the Palestinian coastal plain. This would have been about 1175 BCE. The ‘minimalist’ interpretation argues that the nature of Pharaonic inscriptions needs to be taken into account. Rameses III’s claims are conventional propaganda, either created from a long literary tradition or a compilation building up a minor episode into an earth-shaking threat to Egypt from which the divine Pharaoh delivered her, as was his duty and function. (It should be noted that at least two of Rameses III’s campaigns celebrated in Medinet Habu, his victories over the Nubians and Asiatics, are completely made up [Noort 1994: 108].) The ‘invasion’ of the Sea Peoples was the settlement (perhaps over a lengthy period of time) of a relatively small number of civilians who perhaps arrived as a result of trade links.

Although there are many problems of interpretation, several points about the Sea Peoples emerge with a reasonable probability from the reliefs and inscriptions (cf. Redford 2000: 12–13):

1. They come from ‘islands in the midst of the sea’, which meant Crete and the Aegean archipelago to the Egyptians (cf. the biblical ‘Caphtor’ [Amos 9.7; Jer. 47.4] which seems to mean the same thing [Drews 1998]).
2. They seem to have been well organized and well led (though this does not appear in the reliefs which, by their very nature, picture defeat and chaos). Five (sometimes more) tribes are listed, of which the ‘Philistines’ (*prst*) are only one (see the list above).
3. The presence not just of warriors (the only ones pictured in the sea battle) but also of families and livestock, with household goods in ox carts, suggests the migration of peoples rather than just an army of conquest.
4. A sea battle seems to have taken place in the region of the Nile delta (as *r-h3wt* ‘river mouths’ implies), but the land battle is more of a problem. Some (e.g., Redford 2000: 13) think the land battle was also in northern Egypt, but the inscription refers to ‘Djahy’ which can refer to the Phoenician coast. Three permutations concerning the relationship of the land and sea battles are possible (Ehrlich 1996: 9 n. 45), but the main question relates to the land battle. Ussishkin (forthcoming b) argues that since Egypt controlled Megiddo, the land part of the

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movement would have been stopped in northern Palestine. Ussishkin's view is supported in some ways by the recent book by E. Morris who comments in a footnote:

There is nothing in either the reliefs or the texts that supports the supposition ...that the land battle had occurred near the mouth of the Pelusiac branch of the Nile. Indeed, Ramesses III's referral of the matter to vassals, garrison troops, and maryannu-warriors would argue strongly *against* an Egyptian locale. (Morris 2005: 697 n. 16)

The strength of the Twentieth Egyptian Dynasty is attested by Morris (2005: 703–5) who notes the number of Egyptian fortresses and sites.

This raises a number of questions. Did they arrive to settle primarily by ship or ox cart? If they were opposed by the Egyptians in a crucial fight in the delta region, what about the Egyptian garrisons and general Egyptian control of Palestine to the north? There was apparently a battle at sea, but was there one on land in the same general area – or was the land battle far removed – up north of Megiddo? What material culture is to be associated with the Philistines, and in what periods?

The main indication of the Sea Peoples/Philistines in the coastal plain of Palestine is the material culture. It has been argued that the first phase of locally produced Mycenaean IIIC:1 pottery (sometimes referred to as 'Philistine Monochrome') is to be dated to Rameses III's early reign or the second quarter of the twelfth century, with no direct relationship to Mycenaean IIIA–B imports from LB (Dothan and Zukerman 2004: 43–5). Thus, it cannot be interpreted as an 'import substitution' for wares from the LB Aegean and Cyprus, and 'the abundance of table and kitchen wares in this assemblage demonstrates that Mycenaean IIIC:1 pottery was produced for internal consumption by the population accustomed to Aegean/east Mediterranean culinary practices' (Dothan and Zukerman 2004: 45). It is 'totally different' from the local 'Canaanite' pottery, leading to the conclusion that 'the most reasonable historical background for this phenomenon is the establishment of the new Philistine urban settlement'. Finally, it 'can be suggested that Philistia, Cyprus, and the Aegean maintained close contacts in the first half of the 12th century B.C.E.' (Dothan and Zukerman 2004: 46).

The recent studies of the pottery by A. E. Killebrew (forthcoming; cf. 2000) tell a somewhat different story. At Tell Mique-Ekron, stratum VII shows the sudden appearance of quantities of locally produced Aegean-inspired Mycenaean IIIC Early and associated assemblages. In stratum VI, Mycenaean

IIIC and Bichrome appear together throughout, showing the development from Mycenaean IIIC to Bichrome. Finally, in stratum V, Mycenaean IIIC disappears and Bichrome becomes the predominant decorated ware. One theory, the high chronology (or two-wave theory), would date the Mycenaean IIIC Early (Mycenaean IIIC:1b) to about 1200 BCE, the result of an early proto-Philistine wave of Sea Peoples. The most widely followed theory, the middle chronology (MCC), would date Mycenaean IIIC Early to about 1175 BCE and the Bichrome that developed from it to the mid-twelfth century BCE. This theory is becoming more problematic in light of the increasing consensus that Mycenaean IIIC:1b should be equated to Mycenaean IIIC Early to Middle, which should be dated to the mid-twelfth century. The low chronology theorizes that Mycenaean IIIC:1b appeared only after about 1140 BCE and the date of the Bichrome pottery is even later. Killebrew concludes that, with 'some minor revisions', the low chronology would best fit the dating of Mycenaean IIIC Early–Middle at other sites in the eastern Mediterranean and would also provide a more reasonable dating of Bichrome to the eleventh century (continuing into the tenth). This conclusion is based on the LB II–Iron I stratigraphic sequences at both Tel Mique and Ashdod.

This provides some support for the interpretation of I. Finkelstein (1995b) and D. Ussishkin, that the Philistine settlement was later than Rameses III's eighth year. For example, most recently Ussishkin (forthcoming b) argues that, for all practical purposes, no Philistine pottery – Monochrome or Bichrome – was found at Lachish, even though such pottery was uncovered at the not-too-distant sites of Tel Zafit and Tel Mique. This indicates that Philistine pottery post-dates the Lachish settlement, viz., 1130 BCE. This argument (albeit, one from silence) argues against the 'Mazar–Singer–Stager hypothesis' that lack of 'Philistine' pottery is due to cultural factors. A. Mazar (forthcoming b) responds that Ussishkin's and Finkelstein's arguments that contemporary sites should yield similar pottery assemblages in a geographical zone is correct in principle but should not be rigidly applied when dealing with specific cases such as this. They claim that it is inconceivable that local Mycenaean IIIC pottery did not reach contemporary sites in Philistia and the Shephelah, but this ignores cultural factors that could limit particular pottery to a few urban centres. They think lack of such pottery means a settlement gap, but such a widespread occupational gap is unfeasible and also negated by finds at such sites as Gezer. The early stage of Philistine settlement lasted perhaps only a generation. Lachish is at least 25 km from the major Philistine cities, sufficient to create a cultural border. Ussishkin, however, thinks that the data beg four questions: (1) How could the immigrating Philistines have come down the coastal highway as long as it was guarded by Megiddo? (2) How could the Egyptians have maintained control of Lachish,

Megiddo and Beth-Shean if they did not control the coastal plain? (3) How could Lachish flourish so close to Philistine Gath without any pottery being imported between them? (4) How could Lachish conduct extensive trade with the coastal plain and the ports without any pottery being imported as well? The inevitable conclusion (according to Ussishkin) is that the Philistine settlement of the coastal plain must postdate 1130 BCE.

These differences between archaeologists may seem confusing, but they are no more significant than those between interpretations of the inscriptional data. What we have to recognize are some major divergences between the various models for understanding the settlement of the Sea Peoples and the early history of the Philistines. The archaeology is vital, but it is also very interpretative at certain points. Yet the archaeology also provides data that seem to give a much clearer picture in certain areas. To take an example, Finkelstein (1996b) has compared the settlement patterns in the coastal plain between LB and Iron I, and this appears to lead to more definitive conclusions: in LB the area of the coastal plain that became Philistia seems to have been the most densely populated in Palestine, with many sites of a variety of sizes (about 100, covering about 175 hectares, though there were more sites in the Shephelah than in the coastal plain). About 80 per cent of the Iron I sites had been settled in LB, but there had been a drastic decline in numbers, down to about half the number in LB. On the other hand, the proportion of large sites is up (from about half to three-quarters), while medium-sized sites almost disappear. Even though the number of sites is considerably reduced, it is mainly the smaller sites that no longer exist. Because there are proportionally larger sites (even though fewer), the total built-up area remains much the same. The result is a major reduction in rural sites but a considerable expansion in urban settlement.

Finkelstein goes on to note that the major LB sites were Gezer, Gath, Lachish, Ashkelon, Gaza and Yurza. These continued into Iron I, with two exceptions: Lachish was abandoned (as was Tel Harasim), but Tel Miqne-Ekron had grown considerably. Ashdod and Ekron appear to be the strongest towns in the region, with Ashkelon and Gath the weakest (unless Ashkelon controlled Jaffa). The estimated population is 35,000 for LB and 30,000 for Iron I. This compares with 44,000 for the central hill country in Iron I (but only 2,200 of these were in the Judaeian hills). Considering that few new sites were established in Iron I, and the total built-up area was also very similar, this suggests that the number of new settlers – the Philistines – were few. This confirms what a number of scholars had been arguing: the Philistines settled among the indigenous Canaanite population, perhaps as an elite. One result of this is that the material culture in the region included both Philistine and Canaanite elements (Bunimovitz 1990). But if the Philistines were so few,

this argues against a major invasion force or even a large-scale migration. Whether some portion of the Sea Peoples destroyed the Hittite empire (cf. Singer 2000), Ugarit and other kingdoms along the Mediterranean is perhaps a separate question, though not unrelated.

When we try to track the Philistines into Iron IIA, we run into difficulties because, as L. Herr points out, our archaeological information is poor (1997: 129, 131). We are thrown back on the biblical text with all the problems noted for it elsewhere. If we follow it, the Philistines threatened Israel and Judah. It is probable that the Philistines controlled the Shephelah during this period, but it is unlikely that they were interested in moving into the hill country, since the 'carrying capacity' of the coastal plain had not been reached (Finkelstein 1998c: 24). If there was a clash between Israel/Judah and the Philistines, the former might well have initiated it. But would the highlanders have had the strength or resources to do so? It seems unlikely (cf. Finkelstein 1998c: 24; 2003a: 78). The occasional raid, yes, but not a wholesale invasion of the Philistine territory.

The later history of the Philistines is touched on in the chapters that follow.

3.2.3 Transjordan

Until the coming of the Assyrians, most of our knowledge about the Transjordanian region derives from archaeology and the biblical text. A small amount of information occurs in Egyptian texts (Kitchen 1992; Worschech 1997; see also, with regard to the Shasu, §2.2.1.4), and beginning in the late ninth century or about 800 BCE we have some native inscriptions. The documentation is not great, but it is fortunate that several recent treatments of the archaeology are available (Routledge 2004; forthcoming; Steen 2004; MacDonald, Adams and Bienkowski [eds] 2001; Finkelstein 1995a; Bienkowski [ed.] 1992; Edelman [ed.] 1995a; Miller 1991; Sawyer and Clines [eds] 1983). This includes the important sites of Tall Hisbân (Geraty and LaBianca [eds] 1987–98) and Tall al-‘Umayri (Herr *et al.* [eds] 1989–2002). Much of what we know about the archaeology depends on surface surveys since only a relatively few sites in the areas of ancient Ammon, Moab and Edom have been excavated. As already noted (§1.2.3.3), surface surveys have certain limitations. This is evident in the Transjordan surveys since they have yielded surprisingly different results in some cases, especially disconcerting when this involves overlapping areas in the surveys. These can generally be explained (Miller 1992: 79–80), but it illustrates some of the problems.

In his well-known survey of the Transjordan, Nelson Glueck reached some conclusions that are still influential (see the references and discussion in Sauer

1986). His two main conclusions were (1) that the southern Transjordanian region showed a settlement gap between EB and the end of LB (also between late Iron II and the Persian period) and (2) that a chain of defensive forts marked the borders of the three kingdoms already in Iron I. Sauer questioned the first of these conclusions, arguing that more recent research showed major tell sites in the MB and LB in northern and central Transjordan, suggesting a system of city-states similar to those on the west side of the Jordan. The second of Glueck's conclusions has been widely criticized: the evidence for a chain of border fortresses is skimpy at best (Miller 1992: 79, 87–8; Steen 2004: 80–1).

In the twenty years since Sauer wrote, his conclusions have themselves been subjected to scrutiny, especially since, in the meantime, a number of surveys have reported. Many scholars now feel that Glueck had it largely correct: only a sparse sedentary population was present in MB and LB, but there was an explosion of settlement in the early Iron Age (Miller 1992: 80; Dearman 1992: 72–3; Steen 2004: 89–90). The problem seems to be the interpretation of the surface surveys, for the excavated sites have generally shown little in the way of LB settlement. The indications are that there was a greater sedentary population in the north: in general, the density of settlement decreases as one moves from north to south in Transjordan (LaBianca and Younker 1998: 406). Glueck's southern gap in LB (south of the Wadi Mujib) largely remains: not that there were no inhabitants but that the population in that region was almost entirely nomadic.

Borders are usually difficult to determine from archaeology, and the borders indicated in the biblical text cannot be considered as necessarily accurate (MacDonald 1999: 30–9; Bloch-Smith and Nakhai 1999: 106–7). Yet the textual indications for the locations of Ammon, Moab, and Edom are reasonably consistent around a core region in each case. New Kingdom Egyptian texts already have references to Moab (Rameses II), Edom (Merneptah) and Seir (Rameses II) (Kitchen 1992; Miller 1992: 77–8; Ahituv 1984). Ammon seems to appear first by name in an eighth-century inscription of Tiglath-pileser III (Tadmor 1994: 170–1 [Summary Inscription 7:10]); although it has been proposed that some settlements are mentioned in Egyptian texts, this has also been doubted (Steen 2004: 11–12, 50).

Ammon is associated with the section of the Transjordanian plateau south of the Jabbok River (Wadi az-Zarqa) (Steen 2004: 50–1). At its largest extent it was perhaps the territory between the Jabbok and Arnon (Wadi al-Mujib) and between the Jordan and the desert to the east (MacDonald 1999: 38–9). The southern border probably fluctuated as the fortunes of Ammon and Moab waxed and waned. Although in Ammon the settlement gap inferred by Glueck has been shown not to exist, since several fortified sites have

subsequently been excavated (Tall al-Umayri, Tall Sahab, Tall Safrut, Khirbat Umm ad-Dananir), the LB settlement was nevertheless sparse (Bienkowski 1992a: 5–6; Steen 2004: 51). The existing settlements centred on Amman and the Baq‘ah Valley (Bloch-Smith and Nakhai 1999: 109–11); this area seems to have been the end of a trade route and served as a trade market (Steen 2004: 297–8). The end of LB saw an increase of settlements, but they tended to be small and fortified (Steen 2004: 304). At the transition into Iron I, however, some of the major sites were destroyed or suffered reduction, perhaps as a consequence of the collapse of the market system and trade with the Jordan Valley (Steen 2004: 305–6). Then, many sites were abandoned or even destroyed at the end of Iron I. For example, ‘Umayri was apparently destroyed by hostile attack and extensively reoccupied only in the seventh century BCE. Two-thirds of excavated sites show a gap in late Iron I, though it has been suggested that this is a scholarly artifact resulting from poor pottery chronology or survey information (Bloch-Smith and Nakhai 1999: 111). Herr notes: ‘The early centuries of Iron II are presently very difficult to document in all of Transjordan’ (1997: 132). Little has been found that can be related to the tenth century, which has implications for the biblical passages relating David and Ammon.

Moab seemed to have been focused on Wadi al-Mujib (Bienkowski 1992a: 1). Its southern border is usually seen as Wadi al-Hasa, but most of the places associated with Moab are in fact north of the Mujib. Moab’s northern border probably moved up and down, depending on the balance of power in Ammon and Moab’s relationship. Although the settlement of LB is meagre overall, there may have been an increase in settlement in the area south of Wadi Mujib; in any case, there was a sudden growth in fortified settlements at the beginning of Iron I (Steen 2004: 305), with a steady increase of occupation through Iron I to a peak in Iron II (Bienkowski 1992a: 5–6; Miller 1992: 80; Bloch-Smith and Nakhai 1999: 114). Others assert that walled settlements were infrequent in LB and Iron I, but with fortified outposts and watchtowers reaching a high point in Iron II (Dearman 1992: 73; LaBianca and Younker 1998: 407). It has been argued that the archaeology of Iron IIB reflects the situation described in the Mesha Stela, though the incompleteness of the data at present is admitted (Harrison and Barlow 2005: 184–5, 188). The radiometric dates determined from Tall Madaba samples cluster either side of the date 850 BCE, which well fits the time of Mesha and his activities.

The area of Edom was originally south of Wadi al-Haba (perhaps down to Wadi Hisma) and east of the Wadi Arabah (Bienkowski 1992a: 1). Sauer (1986) attempted to show that Glueck was wrong about the sparseness of settlement in this region in the Late Bronze and early Iron Ages; his efforts have been both rejected (Hart 1992: 93) and accepted (Finkelstein 1995a:

135). MacDonald (1994: 242) noted that there was no LB evidence and also Iron I evidence only in the northern part of the traditional Edomite territory to the east of the Wadi Arabah. Since the occupation of the main sites of Busayrah, Tawilan and Umm al-Biyara showed no settlement before the seventh century BCE, it was possible that the 'Iron I' sherds were actually from Iron II (Bienkowski 1992b; Bloch-Smith and Nakhai 1999: 114). This has initiated an on-going debate between Bienkowski and Finkelstein, who has argued that Glueck was right and that there was continuous Iron Age settlement in southern Transjordan.

Bienkowski (1992b; 1992c) argues that Finkelstein has mistakenly dated sherds to Iron I by using parallels from Palestine and northern Transjordan, and that he has further ignored the fact that Iron II strata lie directly on the bedrock in many excavated sites (though Busayrah has not been sufficiently studied to say for certain). Finkelstein responds that the dating of Edomite sites has depended on C. M. Bennett's chronological sequence based on the find of a seal impression at Umm al-Biyara with the royal name 'Qos-Gabr', an Edomite ruler known from the inscriptions of Esarhaddon and Ashurbanipal (Finkelstein 1992a; 1992b; 1995a: 131-2; also Bienkowski 1992b: 99; Levy *et al.* 2005: 132). The stratigraphy of Edomite sites is far from clear, and it was the highland custom to remove earlier material in order to establish walls on bedrock. The differences between these two scholars hinges a good deal on pottery assemblages: a debate for archaeologists to carry on. For example, a recent study of the collared-rim pithoi (a pottery type appealed to specifically by Finkelstein) concludes that the pithoi are found in Iron II at al-'Umayri and are thus not diagnostic for Iron I (Herr 2001). In any case, the two different interpretations lead to different reconstructions of early Edomite history.

All seem to agree that in LB the inhabitants were nomadic groups whom the Egyptians referred to as *shasu*. According to Bienkowski (1992a: 8) and others, this would have continued through Iron I as well (except for some settlement in northern Edom) before a major process of sedentarization in the eighth or seventh centuries (though the extent of mountainous terrain meant that it did not become fully urban). The resumption of mining in Faynan may have helped with this, as well as the Arabian trade and the general conditions of peace and stability under Assyrian domination. According to Finkelstein (1995a: 135-7) settlement in Edom was parallel to the settlement of the hill country in Palestine and northern Transjordan, though the southern Transjordan was more marginal and yielded small sites. The Edomite state of Iron II had its roots in the sedentary occupation of Iron I. The settlement history was thus not particularly different from that of Moab and the northern Transjordan. All seem to agree that the peak

of settlement was in the eighth to seventh centuries and allowed Edomite influence to spread west of the Wadi Arabah.

Now a new perspective has been given by excavations at Khirbat en-Nahas and the application of ^{14}C analysis to samples taken from slag heaps (Levy *et al.* 2004; Levy *et al.* 2005; Higham *et al.* 2005). They point to two crucial physiographic attributes: (1) an abundant ore deposit that allowed major copper mining and production and (2) a landscape divided between highlands, with some sections having sufficient rainfall to provide fertile agricultural land, and lowlands that could be farmed only with irrigation but also containing the copper deposits. The debate about early Edomite chronology seems to have been partially settled by radiocarbon dating (so Levy *et al.* [2004] argue) because their sampling shows that copper-working at Faynan took place at least as early as the tenth century BCE, with an expansion from about 950 BCE (Higham *et al.* 2005: 177). This metal production occurred over a century or a century and a half but ceased by the end of the ninth century. Levy *et al.* (2005: 160) argue against Finkelstein (2005c), making the point that the fortress at the site is associated with metal production dated to the mid-ninth century at the latest and thus pre-Assyrian (with contemporary radiocarbon dates found at satellite sites) and that the fortress at Khirbat en-Nahas did not exist in the eighth century when the Assyrian palace was extant at Busayra. But their argument relates primarily to Finkelstein's LC thesis in general (though they admit that the sample size is too small to resolve all of the issues of the LC [Levy *et al.* 2005: 158]). In fact, the early dating of metal-working at Faynan seems to provide some support for Finkelstein in his debate with Bienkowski over the settlement of Edom in the early Iron period (Levy *et al.* 2005: 158).

However, some of the conclusions of Levy *et al.* have been strongly critiqued by E. van der Steen and P. Bienkowski (2005; 2005–2006; 2006). One area of particular focus is the technical side of ^{14}C dating and Bayesian analysis. They argue that the figures of Levy *et al.* look to be skewed to give a result about a century earlier than normal calibration would lead to. Attempts at clarification by Levy *et al.* have not laid these concerns to rest. A further question concerns the conclusion from the Khirbat en-Nahas excavations that 'perhaps the emergence of the kingdom of Edom ... began some 200–300 years earlier than previously assumed' (Levy *et al.* 2005: 157). This seems to ignore the level of settlement evidence known from actual excavations and surveys (Herr and Najjar 2001: 338; Bienkowski 1992a: 3). The existence of metal working does not contradict the other archaeological evidence nor necessarily imply a complex social organization in Edom at this time. E. A. Knauf – who had anticipated some of the Khirbat en-Nahas Project's findings – argued that the Faynan mining concern was

controlled from west of the Wadi Arabah during Iron I (1992: 49), a point also made by van der Steen and Bienkowski (2005; 2005–2006). Neither do the new radiocarbon dates have to confirm the statement about Edomite chiefs in Gen. 36.31 (Levy *et al.* 2005: 158; similarly Finkelstein 1995a: 136). The argument that Genesis 36 is a description of the situation in the seventh or sixth century is still a strong one (e.g., Knauf 1992: 49). The new information from Khirbat en-Nahas is to be welcomed and will no doubt have implications for the history of Edom, but it has to be evaluated with other evidence. It is still too early in the debate to start drawing far-reaching conclusions.

The Transjordan in Iron IIB and beyond is discussed in subsequent chapters.

3.2.4 From Settlement to Statehood

3.2.4.1 Joshua and Judges

The book of Joshua describes how the Israelites crossed the Jordan (Joshua 1–5) and conquered the land in only five years, the major cities taken being Jericho, Ai, Makkedah, Libnah, Lachish, Hebron, Debir and Hazor (cf. also Josh. 12.9–24), while Gibeon and Kiriath-Jearim submitted without a fight (Joshua 9). Some decades ago the broad picture of the conquest of Canaan by the Israelites was widely accepted in parts of scholarship, especially in North America, where the ‘Albright hypothesis’ was very influential (see §3.2.4.2). The surprising thing is that when the reader moves from Joshua – where the land was conquered and divided up – to Judges, there is something of a shock, since the land seems far from under Israelite control. Granted, here and there are statements in Joshua that suggest that everything was not conquered all at once (e.g., Josh. 17.14–17), which means that something of a mixed message comes across. Nevertheless, the dominant impression is that it was all settled after five years. This is especially the impression left by Josh. 11.23; 14.15; 21.41–43 and Joshua 13–19 where the land is divided up among the various tribes. The ‘Albright’ or ‘conquest’ hypothesis is generally not accepted anymore, and the question to be asked is whether any of the book of Joshua can be taken as historical. Many would now answer this in the negative (e.g., Na’aman 1994c: 249–81; Van Seters 1990; Miller 1977). Some lists seem to relate to a much later time and situation (cf. Na’aman 2005 on Josh. 15.21–62; 18.21–28; 19.2–8, 40–46).

Judges comes across quite differently from Joshua: far from being conquered and under Israelite rule, the lowlands and many of the main cities

are still controlled by the Canaanites who fight with chariots of iron (Judges 1). As noted above, if one had read Joshua for the first time and then moved to Judges without knowing anything about its contents, it would produce considerable consternation, because a number of the things supposedly accomplished in Joshua have to be done again (e.g., Judg. 1.19-36 versus Joshua 16-19). The book of Judges is mainly made up of a series of episodes which follow a common pattern: Israel sins, is punished by being made subjects of a foreign people, cries to Yhwh, has a deliverer sent who leads them in throwing off the the foreign yoke – following a 40/80 year cycle. This structure is clear through much of the book. These stories, in addition to their entertainment value, had an important moral content, which is probably the main reason they were told. But our concern is with their historicity.

The general picture of Judges has often been seen as an authentic representation of pre-state Israelite society, and this may indeed be the case. As has long been recognized, the narrative of Judges is divided between heroic deliverers ('major judges') and civic leaders ('minor judges'). The latter get little actual space but are presented in two brief lists (Judg. 10.1-5; 12.7-15). Shamgar ben Anath has a curious name, because it suggests a worshipper of the goddess Anat (Judg. 3.31; 5.6; cf §4.2.5). The judge named Jerubbaal is only later identified with Gideon (Judges 6-8; 6.32 gives a nonsensical etymology of his name; it means something like 'Let Baal be great'). Abimelech (son of Gideon) looks like the king of a Canaanite city-state (Judges 9). Jephthah occurs in the list of 'minor judges' but also appears as a heroic figure, whereas Samson seems independent of the other deliverer stories. One cannot rule out that some actual historical core can be found in the 'deliverer' stories, but when we turn the statement around, demonstrating such a core of history is very difficult. B. Halpern (1988) argued for such a core of historicity in the Ehud/Eglon story, but E. A. Knauf (1991) has cast considerable doubt on his argument by showing that 'Ehud' was a Benjaminite clan, and Eglon a town in the Judaeen foothills. Despite references to 'all Israel' only one or two tribes are normally involved in the action in an episode; the Song of Deborah (Judges 5) is the only passage with more than two tribes.

It has often been argued that the 'Song of Deborah' (Judges 5) is an example of early poetry (Robertson 1972), which would suggest that it was written close to the events described (Stager 1988a; see the summary of arguments in Lindars 1995: 212-17). The list of Israelite tribes differs in several ways from all other lists (Machir instead of Manasseh [5.14]; Gilead instead of Gad [5.17]; Judah, Simeon and Levi are absent), which might demonstrate an independent tradition and one possibly earlier than other traditions. Nevertheless, a number of scholars have argued that the

Song of Deborah shows signs of lateness (e.g., Diebner 1995; Lindars 1995: 213–15), and assuming that it is more trustworthy as a historical source is misplaced confidence. P. D. Guest has recently argued that far from being a compilation of different sources, the book of Judges shows the marks of unitary authorship that produced a ‘crafted history’ of the period: ‘Although the text presents itself as history, it should not be mistaken for such’ (1998: 61). In one area, however, Judges may reflect an older linguistic usage: the title ‘judge’ (Hebrew שופט *šôfêṭ*). Although the word means ‘judge’ in a judicial sense in most Hebrew usage, the reference to an individual in Judges means something like ‘political/military leader’ (Niehr 1994). Thus, here and there may be reliable early traditions, but demonstrating them is difficult; the book is generally too problematic to use as a historical source.

3.2.4.2 The Settlement: Recent Models in Biblical Studies

Over the past 30 to 40 years a great deal of debate has centred on the question of how Israel got into the land and eventually became a nation (see the survey in Ramsey 1981; McDermott 1998; Killebrew 2006). Until the 1960s most scholars favoured one of two models, the ‘conquest model’ of the Albright school and the ‘peaceful infiltration model’ of the Alt–Noth school. Then, in 1962 a programmatic article by G. E. Mendenhall in a semi-popular journal outlined a third theory, but its real development came in the late 1970s: the Mendenhall–Gottwald theory of an internal revolt. Much debate has centred on these, and understanding them is still important for getting at the historical situation

The Albright model of a unified conquest was heavily influenced by the biblical text, though it would be incorrect to conclude that it was only a paraphrase of Joshua. Albright initially dated the conquest to the fifteenth century BCE, but then N. Glueck conducted a survey of the Transjordanian region in the years 1938–40 (§3.2.3). Since Glueck concluded that these regions were uninhabited until the thirteenth century, Albright redated the conquest to the thirteenth century, a position he continued to maintain until his death. Although ostensibly based on archaeology, it had an uncanny resemblance to the first half of Joshua. In some ways, this was more apparent than real, but it allowed the conservative tendency to maintain academic respectability while still really following the biblical text. In 1977 J. M. Miller gave a thorough critique of the Albright position and ultimately rejected it (Hayes and Miller 1977: 254–62, 270–7; also Miller 1979). His conclusions presented a picture emphasizing the development of Israel from internal populations and also from the Sea Peoples or tribes forced to

migrate in their wake, and expressed some scepticism toward the significance of nomadic incursions. The position continued to be stoutly defended by Y. Yadin, even in one of the last of his articles to be published (Yadin 1979; 1982), but by then it was being widely abandoned by scholars. There have been a few die-hard defences by conservative evangelicals (e.g., Wood 1990; cf. Bimson 1981 discussed below). Thus, one is somewhat disconcerted to find a mainstream archaeologist trot out the thesis again, apparently without realizing that biblical scholarship has abandoned the position (Ben Tor 1998, but cf. Ben-Ami 2001). The 'conquest' model is now only of historical interest, but it should alert scholars to the fact that vociferous adherence by large numbers of academics is no guarantee that a particular theory will stand the test of time.

Brief mention should be made of John Bimson (1981) who also attempted to resist the rapidly disappearing invasion under Joshua. The views of Albright and others became dominant in their interpretation that Joshua came at the end of the late Bronze Age. Bimson's main contribution to the debate was to show how fragile the archaeological support was for the Albright thesis, although his aim was to undermine Albright's dating rather than the conquest model as such. He wanted to date Joshua to the end of the middle Bronze Age, as Albright originally believed, but his thesis has not gained any real following.

The 'peaceful infiltration' model of Alt and Noth has better stood the test of time in certain ways than Albright's. It continued to find adherents among archaeologists in the 1980s and even the 1990s (e.g., Zertal 1994). Yet a number of criticisms had been levelled at it that rendered the thesis in its original form untenable. One was that the desert could not produce sufficient nomads to populate the hill country in Iron I (see Chaney 1983; but this is disputed by Finkelstein 1988: 308). Another was that nomads adopt this lifestyle out of necessity and want nothing more than the chance to settle down: in fact, the 'nomadic' mode of life takes many forms, and those who engage in it are no more likely to change than those in other forms of subsistence agrarian activity (see the discussion in Lemche 1985: 136–47).

The 'Mendenhall–Gottwald thesis' is widely used to designate the internal revolt model. Gottwald's 1979 study was the academic underpinning for Mendenhall's programmatic thesis almost two decades earlier. A collection strongly espousing the Mendenhall–Gottwald thesis was notable for the trenchant essay by Mendenhall attacking and disowning Gottwald's approach (Mendenhall 1983: 91–103). Although noting some of the genuine differences between the two scholars, it has not stopped the hyphenated designation for this model. N. P. Lemche, while expressing his appreciation

of Gottwald's use of sociology, presented a major critique of his work (1985; also Finkelstein 1988: 306–14). Although differing from Gottwald on many points, including the concept of an internal revolt, Lemche's final picture was of an internal development of the indigenous population. In his view, though, it was not urban residents who fled oppression of the city-states but members of the rural peasantry who went up into the hill country to escape taxes (Lemche 1985: 427–32).

The three models that dominated the twentieth century are no longer equal alternatives. An examination of their strengths and weaknesses has already suggested which way scholarship has moved. The Albright thesis of a unified conquest has all but been abandoned by mainstream critical scholarship and continues to be pressed only by a few conservative scholars. The reason is that much of the support has simply evaporated, in particular the evidence that was once seen as its main strength: the archaeology. In the past quarter of a century most of the discussion has revolved around the Alt–Noth and the Mendenhall–Gottwald theories and has focused on archaeology and on social-scientific models. Neither model is seen to be adequate in itself, and much of recent thinking has combined aspects of both.

A number of variants of these three have subsequently been developed. The 'symbiosis theory' of V. Fritz (1981; 1987) is really a variant of the Alt–Noth thesis, though it also incorporates insights from Mendenhall–Gottwald. Fritz points to the similarity of the material culture of the highland settlers to that found in the territories of the Canaanite city-states, but also differences. This shows that the new population (assumed to be pastoralists) was not the same as the Canaanites but was in close contact with them over several generations. This population eventually settled down, their material culture showing a lot in common with Canaanite culture but also with enough differences to distinguish them.

I. Finkelstein produced what seems to be a true merging of Alt–Noth and Mendenhall–Gottwald, with a firm archaeological base (Finkelstein 1988). Much of his study is on the archaeology of developments during Iron I and documents the unprecedented growth of population in the hill country through this period. Although accepting that the new population included a number of elements, he especially argued that the spectacular growth came about because a large nomadic population settled down. The nomads did not come from the 'deep' desert region, however, but were the descendants of those who left settled life for a pastoral lifestyle in the Middle Bronze Age because of adverse conditions. He argues that the region went through a regular long-term cycle of people moving from settled life to nomadic life and back again, depending on climatic and economic cycles. Thus, the new population of the hill country was made up of nomadic pastoralists

different from the Canaanites (as Alt–Noth proposed) but they were part of the indigenous population (as argued by Mendenhall–Gottwald). He notes that the earliest settlements in the areas of Ephraim, Manasseh and Benjamin were on the desert fringe; they also often settled near Canaanite cities (Finkelstein 1988: 310).

Finkelstein's thesis has been critiqued mainly on the basis that the archaeology of the Iron I hill-country settlements does not indicate nomadic influence on the material culture (Dever 1998a; 1992a). Also, the studies of pastoralists indicate that pastoralism is often not an exclusive lifestyle but one activity of people who also engage in agriculture (Lemche 1985: 136–47; Killebrew 2006: 565). This is true, but it does not strike at the heart of Finkelstein's proposal. More important are studies of Palestinian nomads in recent times who lived and moved freely among the settled areas and peasant farmers. Dever refers to these in his criticisms of Finkelstein (Dever 1998a: 222–9). As G. Lehmann (2003: 155) points out, however, 'it is impossible to estimate the number or the impact of nomadic pastoralists in Iron Age Judah'. More important is the context argued for by Finkelstein: the alternating cycles of sedentary agriculture and pastoralism that he sees as determining settlement and the lifestyle of the inhabitants in Palestine from at least the third millennium BCE.

N. Na'aman (1994c) associates the origins of Israel with the wider developments in the eastern Mediterranean. The thirteenth to eleventh centuries brought the settlement of peripheral areas contemporary with the collapse of urban culture in the entire Aegean–Anatolian–Syro-Palestinian region and the migration of large groups on the boundaries of Mesopotamia. At no other time was the disruption of urban culture in Anatolia and Syro-Palestine (reaching as far as the Aegean and the Balkans) so complete as in the twelfth century. The Arameans gradually took over large tracts in Mesopotamia and Syria. Large-scale migration in Iron I because of destruction in Asia Minor means that various groups reached Canaan and played an important part in settlement: Hittites in Hebron; Hivites in western Benjamin and perhaps around Shechem; Jebusites from the Hittite empire; Gergashites from Anatolia. Only two of the alleged seven pre-Israelite nations were autochthonous. These groups may have helped break Egyptian rule. Traditions about patriarchal migrations to Egypt are best understood against this background; similarly, stories of coming out of Egypt such as the exodus should be understood as vaguely remembered background rather than routine migration of pastoral groups. Thus, the overall picture does not support the assumption that the Iron I settlement was only an internal Palestinian one or settlers only from local Canaanite elements. The various groups entering Canaan joined the local uprooted and pastoral groups, which led to an increase in pastoral and

bandit splinter groups that upset the urban–nomad balance and induced the nomads to settle down: ‘The model that emerges from my analysis is of small and larger groups of variegated ethnic and cultural background who settled during a long period and slowly and gradually started cooperating in the new environment’ (Na’aman 1994c: 246).

Z. Herzog (2002: 89–92) draws attention to the current ‘fluid’ concept of ethnicity that undermines attempts to identify ethnic borders. In the Beersheba valley in Iron I a variety of different groups seems to have lived (according to the biblical text), with a relatively uniform material culture. Here the groups mixed and combined in a complex social composition. Using the Beersheba valley as a model, Herzog argues that the larger settlement and the emergence of an Israelite identity should be understood from an ‘interactive-combined model’ in which ‘a community identity is created from the development and combination of various social groups. Clearly, in different regions there were different combinations of communities, simultaneous with inter-regional mixing and blending’ (2002: 92).

A. E. Killebrew (2005: 149–96; 2006) has argued for the origins of Israel as a ‘mixed multitude’. Although she gives a succinct and persuasive argument for her case, she admits that the thesis is not original to her. Indeed, it has been adumbrated in some form or other (though often briefly and without supporting argumentation) by a number of researchers going back at least to the mid-1980s (Ahlström 1986: 57–83; cf. Dever 1992a; Finkelstein and Na’aman [eds] 1994: 13–14). According to this theory, the population that became Israel was made up of a diverse group of people:

These inhabitants most likely comprised different elements of Late Bronze Age society, namely, the rural Canaanite population, displaced peasants and pastoralists, and lawless *‘apiru* and *shasu*. Outside elements probably included other marginal groups, such as fugitive or ‘runaway’ Semitic slaves from Twentieth-Dynasty New Kingdom Egypt ... Other nonindigenous groups, such as Midianites, Kenites, and Amalekites, perhaps connected with the control of camel caravan trade routes between Arabia and Canaan, may have constituted an essential element of this ‘mixed multitude.’ (Killebrew 2006: 571)

The recent study of E. van der Steen (2004: 306–10) lends support to this thesis. She argues that the trading collapse in Ammon caused inhabitants to migrate not only into Moab but also into the Jordan Valley. There the pressure of population caused further migration into the hill country west of the Jordan. It may be that there was a partial vacuum there at this time because Merneptah (or Rameses II) had ‘laid waste Israel’ (Merneptah Stela) who may have been living in the northern hill country.

3.2.4.3 'Tribes' and 'Nomads'

Several issues debated among social anthropologists have been intertwined with the discussions on the origins of Israel, including 'tribes/tribalism' and 'nomads/nomadism'. First, 'tribes' and 'tribalism' are an aspect of the debate over ethnicity (Rogerson 1978: 86–101; Lemche 1985: 202–44; McNutt 1999: 75–94; Gottwald 1979: 293–341). 'Ethnic group' is now often used where 'tribe' would have been used in the past. 'Tribe' is of course the English word but is frequently the translation of Hebrew שבט (*šēvet*) and מטה (*matteh*). The term has been widely discussed in anthropology (Helm [ed.] 1968; Fried 1968; 1975; Kamp and Yoffee 1980: 88–9; Lemche 1985: 202–44; Khoury and Kostiner [eds] 1990) without any consensus, except that the term is problematic. M. H. Fried (1968: 4–5) stated that the 'single most egregious case of meaninglessness' was the word 'race', but 'tribe' was not far behind: thus, many anthropologists avoid using the term. Yet because of its frequent application in the context of Hebrew Bible study, the word 'tribe' cannot be avoided. Many seem to agree that ancient Israel was a segmentary society, that is, one organized according to (supposed) descent and kinship (but see Rogerson 1986, though he suggests that Israel might have been closer to a segmentary state). The relationship between various Israelites (and also other peoples) was expressed by the creation and use of genealogies (Wilson 1977; Johnson 1988; Levin 2001). In most societies, ancient and modern, genealogies do not necessarily show actual biological relationship or descent but social relationships between members and sections of society. Biblical genealogies also seem to have had this function.

A theory once popular among biblical scholars was that pre-monarchic Israel was organized as an 'amphictyony' (Noth 1930; 1960: 85–109; Gottwald 1979: 345–57), a tribal league organized around a central shrine. After considerable critique of the thesis in the 1960s and especially the 1970s (e.g., Geus 1976; Mayes 1974; Lemche 1977), this model was generally abandoned. Both it and Gottwald's alternative model placed a great deal of emphasis on tribes and a specific confederation of Israelite tribes (Gottwald 1979: 358–86; but see the more cautious view in Gottwald 1993: 177–82). One of the problems is making the tribe the key social unit. Apart from the book of Judges, the text gives little collective function to the tribes. Most collective activity is carried out by small groups, and the tribes appear to be mainly territorial units (Weippert 1973; Geus 1976: 211; Lemche 1985: 71). That is, references to tribes were usually a reference to geography; this clarifies most of the Bible references to Israelite tribes. The people within a specific territory were generally assumed to be of a common lineage, but since property was

seen as belonging by right to a specific lineage (Lev. 25.13-16, 23-28), the two went together.

The essential problem is that 'tribe' has come to be used of social and/or political units of different sorts. The difficulty is not unique to English usage but extends to the biblical text itself. A variety of terms are used in Hebrew to designate divisions of the Israelites, and their significance is widely assumed to be the following (Rogerson 1978: 93-4): שבט (*šēvet*); מטה (*matteh*) 'tribe'; משפחה (*mišpāhāh*) 'family, clan', בית-אב (*bêt-'āv*) 'extended family'. Yet the terms are often used interchangeably, as Rogerson points out (cf. Exod. 6.16-25; Judg. 17.7; Amos 3.1). Furthermore, an entity that would be called a tribe in the modern context of any east African ethnic group would correspond to the entirety of the Israelites, not the individual members of the 'twelve tribes' (Mojola 1998: 26). What this means is that we should be careful about making any social assumptions about the individual Israelite 'tribes'. It seems likely that the system of twelve tribes in the biblical tradition is a late development, perhaps as late as the sixth century (Macchi 1999: 272-80). M. Noth's (1930; 1960: 85-7) analysis of the various lists, showing two types of lists (one with Levi and Joseph, another without Levi but with Ephraim and Manasseh instead of Joseph), has been quite influential. More recent study confirms that there are two systems, but argues that they have been edited together into the scheme of twelve tribes; that is, the twelve-fold system is a late development (Macchi 1999; Kallai 1995; 1999).

Associated with the question of tribalism is that of nomads and the nomadic lifestyle. Through much of the twentieth century many biblical scholars saw the origin of Israel from a nomadic lifestyle (or 'semi-nomads'). The assumption of the Alt-Noth school (§3.2.4.2), but also others, was that a nomadic existence explained much about the early Israelite traditions (with special emphasis on their religion [§4.2.5]). One of the critiques coming from those who embraced the Mendenhall-Gottwald thesis was that nomadism was not the key to understanding early Israel (Chaney 1983), but the place of 'nomads' (or 'pastoralists') in the origins of Israel has not been given up (see the discussion of Finkelstein above [§3.2.4.2]). But now there is a much more sophisticated understanding of the question, though some of the anthropological studies one still sees cited have been superseded.

Earlier discussions especially focused on the work of J. R. Kupper, J. T. Luke and M. B. Rowton (see the summary in Kamp and Yoffee 1980: 90-2). Unfortunately, models were developed on these studies by biblical scholars who were apparently unaware of the wide range of nomadic/pastoralist modes of living (Irons and Dyson-Hudson [eds] 1972; Nelson [ed.] 1973; Kamp and Yoffee 1980: 92-4; Lemche 1985: 84-163; Khazanov 1984; Staubli 1991). The main problem was the assumption of bipolar opposites:

sedentary/nomadic, agriculturalist/pastoralist, rural/urban, village/city (Mohammed 1973; Kamp and Yoffee 1980: 93; Lemche 1985: 198–201). This is despite the fact that a number of researchers recognized that pastoralists and farmers generally had a mutually beneficial relationship and usually lived together in harmony. What was often forgotten was that (a) a people might include both pastoralists and agriculturalists, or (b) even the same individuals might carry on both crop-growing and animal-herding, and (c) people could cycle lifestyles, being sometimes pastoralists and sometimes settled.

Some general comments will summarize the discussion:

- The ubiquitous reference to ‘tribes’ in the Bible, including in some possibly early passages, suggest that Israelite society was eventually ‘tribalized’. Yet if Israel and Judah grew out of a coalescing of various elements (pastoralists, agriculturalists, Transjordanians, etc.), ‘tribes’ may have developed later rather than earlier.
- In any case, tribalization seems to have had little practical effect, since we see little evidence that society was organized by tribes for administrative purposes or collective action. The twelve-tribe scheme, known from many biblical passages, is likely to be a late development, created for ideological purposes.
- The once-common assumption that early Israel consisted of a tribal league (whether Noth’s ‘amphictyony’ or something related) is still influential in some circles, even though generally thought to have been refuted.
- If, as will be argued below (§3.3), Israel and Judah developed from a variety of indigenous peoples who inhabited the highlands, it is likely that this mixture included pastoralists and transhumants. Thus, the study of ‘nomads’ in its broadest sense is still relevant to early Israel, but the great variety of types, lifestyles and combinations has to be recognized.

3.2.4.4 Anthropological Models of Statehood

The question of statehood for Israel and Judah cannot easily be disassociated from that of the settlement: the settlement and the rise of the monarchy seem ultimately to be part of the same process. This does not suggest that, once those who became Israel settled down, a state was inevitable, but the settlement – its nature, its development, the resources available – are all determining factors in the progress toward statehood. One followed from and presupposed the other.

ANCIENT ISRAEL

In recent years there has been much discussion among anthropologists and archaeologists about the nature of states, the types of states, how states develop and what precedes them. It is not unusual for discussions about the rise of Israel to make some reference to this discussion, but it must be said that most writings by biblical scholars do not give a thorough treatment of the subject. It is fair to say that when anthropological studies on state development are mentioned, it is not infrequently to expound one and follow it without serious discussion of other possibilities or critique of the one being followed. The literature on the subject is quite substantial, especially if one takes into account everything written since the seminal work by M. Fortes and E. Evans-Pritchard (1940). No attempt will be made here to trace the discussion over the decades, but several points can be made in order to summarize the most important considerations for discussion in the present:

1. Much of the discussion, at least until recently, has been based on evolutionary models (Redman 1999: 49–53; Gosden 1999: 476–82; Renfrew and Bahn 2004: 178–82; Yoffee 2005: 22–41). A popular one has been the model of E. R. Service (1962) in which civilization developed through the stages of bands, tribes, chiefdoms and finally states. Evolutionary models do not have to force everything into a rigid pattern nor suggest inevitability (Feinman and Marcus [eds] 1998: 5–6). N. Yoffee fully accepts the concept of social evolution, but his recent study is a sustained critique of the widely regarded ‘neo-evolutionary model’ (Yoffee 2005: 22–41).
2. The various models of statehood are ideal types, but there is a danger of elevating ideal types to reality. Reality is always much more complicated, while ideal types only serve as a means of investigation and comparison. Within the various types there is a great deal of variation. To pick one sociological model and argue from it without considering others or without critiquing the models themselves is to be naive and uncritical.
3. The concept of chiefdom – so beloved of studies on the development of statehood in Israel and Judah – has been much criticized (Yoffee 2005: 22–31). See further on this below.

The types of early states have been discussed in the anthropological literature, with a bewildering variety of models: city-states, territorial states, regional states, segmentary states, tribal states, ethnic states. The term ‘city-state’ frequently comes up in discussions relating to ancient Palestine, yet a recent collection found the term problematic (Feinman and Marcus [eds] 1998: 8–10). Yet B. Trigger (2003: 92–113) divides early states into the ‘city-state’ and ‘the territorial state’; yet, disconcertingly, toward the end of

his discussion he suddenly introduces the 'regional state/kingdom' without discussing what it is or how it relates to the other two. 'Regional state' is also used by T. J. Barfield (1990), but he seems to be using the term in a different sense from Trigger.

A number of the recent theorists on the rise of the Israelite and Judahite states have worked with the model of the chiefdom. Of recent works R. D. Miller II argues that Israel began as a 'complex chiefdom' (2005: esp. 6–14):

Those having an intermediate level or levels of 'subchiefs' between the paramount and the people ... It is complex chiefdoms that develop into states and that one would expect to find in Israel on the threshold of Monarchy. (Miller 2005: 8)

Three main characteristics are said to be associated with complex chiefdoms: (1) tribute mobilization, (2) cycling (between two or three levels of control) and (3) sacralization. Unfortunately, Miller does not appear to take account of the debate. For example, he makes little reference to the critique of the chiefdom concept (e.g., Yoffee 2005: 22–31) and none to those who argue that the chiefdom is an alternative to the state, that is, that chiefdoms do not develop into states (cf. Yoffee 2005: 26–7). His statement that it is the complex chiefdom that develops into the state also does not seem to recognize that the complex chiefdom might well cycle into a 'simple chiefdom'.

N. Yoffee (2005) both critiques the 'neo-evolutionary' thesis and also makes his own proposals about the development of the state. His thesis seems to be expressed in the following statement:

New social roles and new forms of social relations emerged alongside, and to an extent supplanted, exclusive kinship rules (of marriages and the status of children) that also functioned as the framework for relations of production. Leadership, exercised by shamans, expert hunters, and charismatic individuals, gave way to formalized ideologies in which the accumulation of wealth and high status were seen as rightfully belonging to leaders whose roles were, among other things, to 'make inequality enchant.' As social relations were transformed into relations of domination, new ideologies led to the acquiescence of subjects in their own domination and the production of their own subordination (Godelier 1986). The new ideologies of state, which were inextricable from the changing social relations that gave them birth, thus depicted how dominant leaders 'served' those who daily and perpetually served them ... The earliest states, thus, consisted of a political center with its own leadership structure, specialized activities, and personnel, but also included numerous differentiated groups. These social groups continuously changed in their organization and membership in relation to the needs and goals, strengths and weaknesses of the political center. (2005: 32–4)

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According to Yoffee (2005: 34–8) the critical processes of social and economic differentiation and political integration come about through the various forms of power that had to be in place before the earliest states could evolve: economic power (agricultural production, mercantile activity), social power (elites at both the state and local level, creation or adaptation of symbols of cultural commonality) and political power (bureaucratic administration, military organization, legal system, taxation structure). Although ancient states were unlikely to have passed through a chiefdom-like stage, he accepts and even presses the concept of city-state (2005: 42–62). He mentions in passing ‘Yoffee’s rule’: ‘If you can argue whether a society is a state or isn’t, then it isn’t’ (2005: 41).

A. H. Joffe proposes the model of the ‘ethnic state’ for the rise of secondary states in the southern Levant, meaning Israel, Judah, Ammon, Moab and finally Edom. By ‘ethnic state’ he means:

Politics integrated by means of identity, especially ethnicity, and which are territorially based ... they are novel and historically contingent political systems which appear in the Levant during the first millennium BCE thanks to the confluence of several factors, not least of all the collapse of imperial domination and the longstanding city-state system. (2002: 426)

Joffe focuses on the archaeology and extra-biblical texts rather than the biblical accounts.

Based on fragmented evidence from the material culture (e.g., a series of palatial structures in *bit-hilani* style; proto-Aeolic capitals; red-burnished Phoenician-style tableware) Joffe identifies a state in the Cisjordan region already in the tenth century (2002: 440–6). The similarity of construction styles at Megiddo, Hazor and Gezer indicates they were part of a larger political unit, but all three seem to be in border areas. What the centre might be is not known. In spite of some marks of royal ideology, other significant ones are missing: representational art, monumental inscriptions, inscribed or decorated objects, inscribed or uninscribed seals or weights. Although Joffe insists on calling it a state, he admits it was fragile, hardly integrated and had little in the way of meaningful ethnic unity. In any case, much depends on following the conventional dating: if the LC turns out to be correct, the postulated development of the ethnic states will be somewhat different.

Other types of early state are sometimes postulated. The term ‘segmentary state’ seems to have been coined by A. W. Southall (1956). In a more recent article, J. W. Rogerson argued that ancient Israel was not a segmentary society but approached being a segmentary state (1986). The concept has come under considerable criticism, and Southall himself eventually gave it

up (Marcus and Feinman 1998: 7–8). Another is ‘tribal state’, of which three sorts are proposed: (1) one tribal elite or dynasty conquers and rules over a heterogeneous population; (2) a non-tribal dynasty is brought to power by and depends on tribal support; (3) the rulers attempt to eliminate tribalism but promote a national ideology of integration that resembles tribal ideology (Tapper 1990: 69). Inspired by M. Weber, L. E. Stager (1985a: 25–8), followed by D. Schloen (2001) and D. M. Master (2001), proposed the archetype of the patrimonial state. This is modelled on the household, in which leadership is vested by tradition in the patriarchal figure. In a recent book on the rise of Moab as a state, however, B. Routledge considered but rejected both these popular ‘local models’ of state formation, ‘tribal states’ and ‘patrimonial states’ (2004: 115–23).

One of the points made in a number of studies is the importance of ideology to the founding of states (Caton 1990; also to ethnic identity: §1.2.4). ‘Ideology’ can of course cover a number of different perspectives, including that of religion. For example, E. Gellner (1981) has demonstrated the significance of saints and religious leaders for the development or maintenance of chiefdoms in early Islamic history (also Tapper 1990: 65). In this light, the suggestion of Gottwald and others that Yahwism was important to the development of early Israel takes on a new significance. We also have to consider the place of law, which can play a similar role. In Israel law included not just civil law but moral and religious law. Unfortunately, it becomes a matter of speculation at this point because we have so little evidence, but see further at §3.3.

3.2.4.5 The Saul, David and Solomon Traditions

It has often been assumed in the past that with the reigns of Saul and David we begin to get into the historical period. In Noth’s classic theory, 1 Samuel 13 to 2 Kings 2 was thought to have been made up of several blocks of tradition that were given only minor editing (including some formulaic introductions and transitional passages) before being incorporated into the DtrH (Noth 1981: 54–7). More recent analysis has not been as optimistic about finding early narratives, but the stories of Saul and David show evidence of a variety of traditions: there is a pro-Saul bias in the Saul traditions and anti-David perspectives in the Davidic material. There are also some essential differences between them and the Solomon narrative.

Let us first note some salient points about the Saul tradition. First of all, Saul looks like a chieftain, with a court that meets under a tree (1 Sam. 22.6). We have two versions of how he became king: one is that he was anointed by

Samuel (1 Samuel 9.1–10.16 (23) + 13.2–14.52), which looks like a biased account from a prophetic source that wants to make Saul subordinate to Samuel; the other – more likely to be reliable – is that he arose as a deliverer (1 Sam. 11.1–15). Whether there was an actual Philistine threat in his time is debatable, but it is possible. The extent of Saul's territory was probably the same as that of Eshbaal (2 Sam. 2.9) and included the central highlands. Many of his activities could have been accomplished in two years (cf. 1 Sam. 13.1).

A recent analysis of the Saul tradition finds a historical core, though this has been filtered through the distorting lenses of Davidic court circles, prophetic circles, Deuteronomistic perspectives and anti-monarchic views (Shalom Brooks 2005). The stories of killing the priests of Nob or the Gibeonites are probably later calumnies. According to Shalom Brooks the population in the central highlands was already moving toward a new socio-economic situation characterized by a developing centralization. This was the background for the rise of the monarchy. Saul was a successful leader, the first to develop a standing army, who had the support of the people, including those of the Judahite hill country. Saul was not only able to unite the Israelite tribes but also to incorporate Canaanites and other minority groups into the emerging state. Loyalty to Saul continued after his death, creating rebellions and other problems for David; indeed, David almost wrecked the monarchy by his sabotage of Saul's rule in order to gain the throne for himself.

Similarly, D. Edelman (1996a; 1991) sees the historical Saul as the petty king of Gibeon with Benjaminite roots who expanded into surrounding territory to create a state called 'Israel'. Attempts to control local trade routes and find markets brought him into conflict with the Philistines and other independent states. He died trying to expand into the Jezreel–Beth She'an corridor.

A number of recent studies attempt to analyse the David story (Brettler 1995: 91–111; McKenzie 2000; Halpern 2001; Finkelstein and Silberman 2006). The David traditions are in part bound up with the Saul traditions, and they need to be evaluated together. One of the concerns in the early part of the story is to legitimate David – strongly suggesting that David was a usurper (Shalom Brooks 2005: ch. 4). To take one analysis, N. Na'aman (1996a) argues for some written material (lists, extracts from a chronicle of kings), though not all necessarily from David's time. It was not customary at this time to cite documents. These probably came from the scribal milieu where such documents were used for training. For example, the story of Shishak's invasion was based on an actual account, though it was interpreted by a later writer (but see other interpretations at §3.1.4). The verdict is still out, however, as to whether he ruled over a large kingdom.

In the biblical story, David fits the image of the hero figure; there are many folkloristic elements and a variety of traditions; yet there are also traditions with some interesting twists, such as the willingness to acknowledge some of David's weaknesses, the need to legitimate David from a variety of angles – suggesting that he was not seen as legitimate by everyone – and the admission that David did not do certain things that we might have expected. To summarize, we can note some of the points that emerge from a look at the Saul and David traditions:

- The tradition recognizes that David was not the first king.
- Saul came to the throne probably as a military leader by popular acclaim (1 Sam. 11.1-15), whereas the prophetic tradition that the king was subject to Samuel's choice and censure (1 Sam. 9.1-10.16, 10.23; 13.2-14.52) is unrealistic.
- The apparent boundaries of Saul's kingdom (2 Sam. 2.9) is reasonably in line with the natural and demographic resources in Cisjordan.
- A strong link is made between David's rise and Saul's court, but much of this looks like a deliberate attempt to legitimate David as king from a variety of angles: anointing by Samuel (1 Sam. 16.1-13); armour-bearer in Saul's court, who plays the lyre for him personally (1 Sam. 16.14-23); slaying of Goliath (1 Samuel 17); marriage to Saul's daughter (1 Sam. 18.17-27).
- Contrary to expectations David does not build a temple (though a strenuous effort is made for him to do everything short of the actual building).
- Both Saul and David were mainly military leaders.
- The text itself does not suggest an extensive administrative apparatus in the case of either Saul or David.

The narrative about Solomon's reign (1 Kings 2-11) seems to be rather different from those about Saul and David. One is immediately struck by how uniform it is. Almost from start to finish Solomon fits the image of the great 'oriental emperor'. He controls a vast territory and possesses great wealth, with absolute sovereignty over his subjects. Of course, he marries the daughter of a country of similar power – suggesting equality with Egypt in this case – and harnesses the best craftsmen and materials from legendary Tyre to build his city. His capital city consists of great palaces and a magnificent temple, with gold like dust and silver so abundant it is of little account. His household overflows with luxuries, his table groans under the weight of exotic fruits, meats from rare animals, and every sort of desirable food for consumption. His wisdom is legendary, and he exceeds all others

in intellectual skills. His reputation reaches far and wide, and rulers from distant lands travel to see such a supreme example of power, wealth and wisdom – only to find that the reports were understated. His ships travel to the ends of the earth for rare and astonishing goods.

I find it difficult to discover much in the Solomon story that strikes me as likely to be historical. I do not discount the existence of a king Solomon. His name echoing the old god of Jerusalem (Shalim/Shalem) is suggestive of reality rather than simply the piety of the David story. Also, he began his reign with the bloody elimination of rivals, though the idea that he took his throne in the midst of adversity which he overcame could be a part of the stereotype, and the writer probably saw nothing bad in this. But, overall, I can find little in the Solomon story that looks on the face of it to be historically reliable. Yet I am intrigued by the story that he built the Jerusalem temple. This sort of story is what we might expect, and the description of the wealth and rare construction of the temple fits well the legend. Yet David – the expected temple-builder – did not construct it, and we find nothing in the stories of the later kings that might hide such a building (with the possible exception of Jehoash who is said to collect money to repair the temple: 2 Kings 12). This suggests that a temple was built in Jerusalem at a fairly early time. If David did not build it, who? Possibly here we have a genuine remembrance that has been expanded into a great legend.

E. A. Knauf (1997) thinks that Solomon was historical but that he differed considerably from the biblical picture. The king's name shows that he was non-Judaean in origin. The Bathsheba story was not suppressed because there was a worse story: Solomon was not David's. Knauf has tried to reconstruct some early sources. He sees 1 Kgs 8.12-13 as an early text quoting an incomplete royal building inscription which confirms Solomon's place in association with the temple (though perhaps only establishing a cult in a pre-existent temple rather than a temple builder as such). In 1 Kings 1-2 (a text probably from the seventh century) he sees glimpses of Solomon as the puppet of the Jerusalem elite on whom he turns and whom he eliminates. He concludes that Solomon was the son of a Jerusalem mother but not necessarily of a Judaean father. He became king through a *coup d'état* by getting rid of the Jerusalem elite. He was no monotheist, because the Judaean tribal deity Yhwh had only a subordinate position in the Jerusalem pantheon.

N. Na'aman (1997d) also analysed the account of Solomon for the existence of sources. He accepts that the early 'chronicle of Israelite kings' was created in the eighth century and thus rather later than Solomon; however, the Deuteronomist also had the 'Book of the Acts of Solomon' (1 Kgs 11.41), which Na'aman tries to reconstruct. Some episodes were invented by the post-Deuteronomic redactor, including the Queen of Sheba

and the description of the temple (based on a description of the temple of his own times). Na'aman concludes that only in the late redaction do we have a picture of a ruler of an empire and a great sage. Although I agree that there was a 'Chronicle of the Kings of Judah' (Grabbe 2006b), I am more sceptical of a 'Book of the Acts of Solomon'. If such existed, it might have provided some historical facts, but we could not be sure without a careful analysis. But were such writings being produced this early? Administrative documents, yes, but not the sort of biographical writing envisaged here (§3.2.5). Thus, here and there might be a verse that reflects the historical Solomon, but to my mind the Solomon story is the most problematic, providing the thickest cloud of obscurity over the history that lies behind it.

3.2.5 Writing, Literacy and Bureaucracy

The Proto-Canaanite alphabet is attested as early as the fourteenth century BCE, but the standard view of its development into the Hebrew, Phoenician and Aramaic alphabets of Iron IIA is now contested. A concomitant debate concerns the development of official written texts: seals, inventories, documents, records, diaries and the like. Finally, there is the question of how early and how widespread knowledge of writing was within the wider population.

B. Sass (2005) has recently re-evaluated the development of the Northwest Semitic alphabet. The conventional view had been that the latest Proto-Canaanite inscriptions were to be dated to the eleventh century, followed by the early monumental inscriptions from Byblos in the tenth century (the Ahiiram sarcophagus being placed about 1000 BCE). The earliest examples of the Aramaic and Greek alphabets were also placed in the eleventh century, even though the actual evidence suggested this was two and a half centuries too early (Sass 2005: 14). Sass himself sees no problem with the relative chronology but argues that the absolute chronology should be lowered by about 250 years for many of the diagnostic inscriptions (see the chart in 2005: 73).

Currently, there is a dearth of monumental and other inscriptions that one associates with a functioning state until the ninth century. Sass (2005: 60) argues that the lack of monumental inscriptions is a reflection of the true state of affairs – there were few such inscriptions produced at that time (the lack of finds after so much archaeological work over many decades is definitive). On the other hand, administrative documents have not been found because they were probably written on perishable material. The finds of bullae that once sealed documents is one of the main indicators (Lemaire 1981; Sass 2005: 51; cf. Reich, Shukron and Lemaire forthcoming).

What does this say about the development of a bureaucracy, which in turn is usually taken as evidence of a fully fledged state? We should first be aware that a state can exist without the existence of writing (Trigger 2003: 595–8). Bureaucracy can exist without writing, and a state does not need an enormously complicated bureaucracy to be a state. Ancient Israel and Judah were not Mesopotamia or Egypt. The scribal structure needed to carry out the necessary administrative tasks might have been a fairly minimal one, with scribes trained by apprenticeship rather than in scribal schools, and the scribal office perhaps handed down from father to son (Grabbe 1995: 160–1, 173–4).

On the other hand, the kingdom of Solomon as described in the biblical text would have required an extensive bureaucracy to co-ordinate all the various governmental activities ascribed to his reign. Rather than a sophisticated administrative apparatus at the beginning of Israelite statehood (which is the picture given to us in the Bible), there are increasing signs of the spread of writing and literacy as time goes on, with the climax reached in seventh-century BCE Judah. This ‘explosion of writing’ has been explained as evidence of an increase in the bureaucracy (Finkelstein and Silberman 2001: 270; Stern 2001: 169). The finds indicate a greater quantity of written objects preserved from the seventh century – seals, seal impressions, ostraca and inscribed weights (Finkelstein and Silberman 2001: 270, 280–1; Stern 2001: 169–200) – seeming evidence that Judah had become a fully developed state by this time (Finkelstein and Silberman 2001: 281, 284). Thus, the sorts of written objects catalogued here do look like the type of written material that would be the product of the bureaucracy and state administration, but this was long after the tenth century.

The extent of literacy among the population has been much debated, but in order to understand ancient Judaism fully, we must keep in mind that it was mainly an oral society, like most other societies of the ancient Near East (Harris 1989; Niditch 1996; Young 1998). Although writing was known in both Egypt and Mesopotamia from about 3000 BCE, writing in many cases was quite complicated and primarily in the hands of professional scribes. Most people in the ancient world were not literate, even in cultures with an alphabetic script, and we have no indication that the Jews were any different from their Near Eastern neighbours. Ability to write one’s name or decipher a seal inscription implies a different concept of literacy than the capacity to read, understand and write long documents (cf. Street 1984; Niditch 1996: 39–45). A society can know writing and still be an oral society (Macdonald 2005). If Jewish literacy at the turn of the era was no more than about 3–5 per cent (Hezser 2001: 496), it would have been correspondingly less in a largely peasant population in the early centuries of Northwest Semitic

writing. If there had been lengthy written material, who would have read it?

All examples of writing we have from Palestine in this era are short and functional. Estimates of how early we have inscriptions in Palestine vary. N. Na'aman (1994c: 219–22) pointed out that there were no pre-eighth-century alphabetic writings in the area of Israel and Judah (except for the Gezer calendar which he suggested was probably Canaanite). The spread of alphabetic writing did not antedate the mid-eighth century, and not a single inscription has been found in Jerusalem before the late eighth century. This was more than a decade ago; things have changed but not by much. To the tenth century Renz (1995) attributes the Gezer calendar and three short inscriptions from Tel Batash, Tel 'Amal and Rosh Zayit – to which he adds two brief inscriptions from Beth-Shemesh and Tel Rehov VI. In addition to the four Arad ostraca that Renz attributes to the ninth century, along with the short inscription from Tell el-Hammah and the Kuntilet 'Ajrud inscription, he would include two from Tel Rehov IV (see also Mazar forthcoming a). Even if there had been widespread functional literacy, there was very little written material available.

As already noted above, by the seventh century there was a significant increase in written artifacts, which has been interpreted as demonstrating a greater degree of literacy in Judah at this time. Indeed, it has been associated with the rise of the Deuteronomic movement and the promulgation of Deuteronomy under Josiah:

The very fact that a written law code suddenly appeared at this time meshes well with the archaeological record of the spread of literacy in Judah ... The report of the appearance of a definitive written text and its public reading by the king accords with the evidence for the sudden, dramatic spread of literacy in seventh-century Judah ... Writing joined preaching as a medium for advancing a set of quite revolutionary political, religious, and social ideas. (Finkelstein and Silberman 2001: 280–1, 284)

I am not so sure that the inferences drawn here are justified: the increases are generally those associated with the bureaucracy and scribal bureaucrats. The increase in written material of this sort would not necessarily indicate an increase of general literacy in the population. There are also levels of literacy: the ability to read the short inscription on a seal impression or even an administrative document does not demonstrate the reading of long religious documents. If Deuteronomy was really produced at this time – a view widely held but also disputed (§3.1.6.2) – the writers were naturally literate. But it does not follow that there was greater literacy in the general population.

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There is no evidence of multiple copies of the document or that people were reading it for themselves; on the contrary, it was by public teaching – perhaps including public reading – that it would have been promulgated (cf. Deut. 31.10-13).

The archaeological finds of seals, seal impressions and ostraca give no support to the view that ‘writing joined preaching for advancing a set of quite revolutionary political, religious, and social ideas’. Where are such ideas promulgated in the seal impressions and ostraca? Granted, the apparent move to aniconic inscriptions on seals may be a datum giving evidence of the spread of Josiah’s reform. But the mere presence of aniconic seals is not an overt means of propagating the revolutionary ideals that accompanied such a reform. The move away from iconography would be a consequence of such a reform, not the vehicle for advancing it. The changes in seal impressions would probably have been noted consciously only by a very few.

The much debated issue of schools in ancient Israel (Lemaire 1981; Millard 1985; Crenshaw 1985; 1998: 85–113; Haran 1988; Grabbe 1995: 171–4) includes two separate questions. Did schools exist to teach scribes? The answer is, possibly, but the case is not proved. Although scribes were trained in schools in ancient Egypt and Mesopotamia, the scribal needs for a small entity such as Judah could probably be met by a form of apprenticeship. The second issue is, did schools exist for general public education? The answer is definitely negative. If there were schools for others than scribes, they would have been for the wealthy and aristocratic, though these could probably afford to hire tutors. Apart from scribal schools, schools are first attested in the Greek period.

3.3 Synthesis

The context for this chapter has been the eastern Mediterranean and ancient Near East during the latter part of LB and the beginning of the Iron Age. The end of LB saw a general collapse of trade and communication, with many cities destroyed along the Mediterranean coast and sometimes further inland. The late Bronze Age had been what S. Sherratt unapologetically refers to as a period of ‘globalization’ (Sherratt 2003), which means that the collapse had a far-reaching effect on the various cultures in the region from about 1200 BCE. It was about this time that Egypt withdrew from Palestine, now thought by many to be about 1130 BCE. Concomitant with this was a remarkable increase in settlement in the highlands, mainly small sites. Yet the city-state system of Palestine and Syria did not come to an end as such. The rural sector certainly experienced continuity, with the peasants in the

vicinity of the ruined LB cities living as they always had. Especially in the northern valleys the population remained dense. Then, in Iron I the main urban centres began to recover, including the development of trade with Phoenicia, Cyprus and beyond. In a few cases, even new city-states emerged, especially to replace those that had declined or disappeared.

It was in the time frame of this chapter that we began the history of Israel proper, for in many ways, we can say that 'Israel' begins with Merneptah. Something called Israel existed in the Palestinian region about 1200 BCE, and it appears to have been a people. Exactly where this people lived, what/who constituted it, where it got its name and its relationship to the Israel of the Bible are all questions. Some will say that the answer is obvious, but others will point out that this is not the same as proof. It has long been suggested that a number of innovations in technology took place in the central highlands: terracing, plastered cisterns, the 'Israelite house', collared-rim jars, and the use of iron; also the absence of pig husbandry has been seen as significant (§1.2.4; §3.1.1). Unfortunately, it seems there is no list of technologies or practices that is exclusive to this region and time or that can serve as 'Israelite ethnic markers'. On the contrary, it seems that a variety of ethnic groups (Hittites, Hurrians, Jebusites, Gergashites, Amorites, Shasu [?], etc.), as well as social elements ('*apiru*', Shasu [?], pastoralists settling down, peasants fleeing the lowlands, etc.), settled the hill country on both sides of the Jordan in Iron I, if our written sources are anything to go by. Some of the tribal and other groups known to us from biblical genealogies might have originated in this period, though we know that others are probably much later creations. Local leaders might have given rise to some of the stories in the book of Judges, but it is doubtful if much historical memory remains here, even if the general picture of a disunited series of autonomous (or partially autonomous) peoples is quite believable. Some still invoke the exodus tradition: although mainstream scholarship has long rejected the origin of Israel in this way, there is still a widespread view that some of the Israelites may have come from a temporary sojourn in Egypt. It can be neither proved nor disproved.

Eventually the kingdoms of Israel and Judah developed in this region. What was the process? Perhaps one important factor was a growth in population: a critical mass of settlers was reached without which certain things could not happen, but this was more significant in the north and decreased as one went south, with the area south of Jerusalem still being mainly pastoral. It seems that at some point a dominant ethnic consciousness came about in this region. Part of the reason might be by force: an 'Israelite' group might have conquered or otherwise taken over some other smaller groups and assimilated them (as suggested in Joshua and Judges). But it is

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doubtful if that was sufficient. Here the suggestion of Mendenhall, Gottwald and others might have some theoretical validity, even if they provided no concrete evidence: Yahwism. One of the characteristics – and constituents – of an ethnic group is a set of common myths and ideologies, which can include religion (§1.2.4; §3.2.4.4). Israelite society was long polytheistic, but Yhwh did function in some way as a national god and seems to have been the most widely honoured deity (§4.2.5). Thus, it is possible that Israel partly coalesced around Yhwh but, even if true, this would have been only one factor; it would be simplistic to suggest that this was the only factor. Indeed, we do not know for certain to what extent the hill peoples saw themselves as a single ethnic group, but the question becomes relevant if we start discussing Israel as an ‘ethnic state’.

We can now ask how and why Israel and Judah developed into states, for we almost all agree that they eventually became states. The problem is that the current debate makes it difficult to come to firm conclusions about the archaeology (§3.1.1), especially with the debate over the LC (§1.2.3.4), but the archaeology does put limits on what can and cannot have happened. For many anthropologists, the formation of the Israelite and Judahite states would probably be explained from impersonal social and political forces, but others would allow a personal element into the mix, such as the presence of a dynamic charismatic leader. For example, Coote and Whitlam (1987) explain the rise of Israel as a state from the combination of pressure on highland resources as the population expanded and pressure from the Philistines in the lowlands. This follows a long line of scholars seeing the ‘Philistine threat’ as in some way pressing the Israelites into initiating a monarchy.

Yet there are some objections to these proposals. First, there is no evidence that the ‘bearing capacity’ of the highlands had been reached; in other words, archaeological surveys suggest that the highland population could still have been sustained by local resources without having to expand: the same population or greater could be found there in other periods (cf. Finkelstein 1999a; Broshi and Finkelstein 1992). Second, was there any impetus for the lowlanders to expand into the highlands since they also still had room to grow (Finkelstein 1998c: 24)? Thus, if there was friction between the Israelites and Philistines, it is likely to have been the Israelites who initiated it – perhaps making raids on the Philistines. But considering this explanation inadequate does not answer the question of why the highland settlements coalesced into a state.

By rights we would expect the first secondary state in Palestine to arise in the coastal plain, but here the Philistine city-states were successful, and there seems to have been no impetus to form a larger unit. The next area

would be the northern region, perhaps the northern valleys or hill country. It is precisely in this area that the Omride kingdom emerged, which is one of the reasons that some scholars think this was the first Israelite state. The problem is what to do with the biblical traditions about the rise of the Israelite kingship. The idea that Israel was under divine rule but had Samuel anoint a king chosen by Yhwh is of course pure theology, not history. Should we simply dismiss the whole of the biblical account as too difficult to deal with? This is perhaps tempting, and some have certainly followed this approach. Yet more considered analyses suggest that there is history behind the stories of the 'united monarchy', even if precisely what sort of history is a matter of debate.

I believe there is sufficient evidence to assert that Saul, David and Solomon existed. Saul was evidently the first king, was primarily a military leader, and was overthrown by David who subsequently met a lot of opposition, including from Judah. If anything resembling a united Israel – a territorial state – came about under David, it would have been an unusual development. One can hardly claim that it would be impossible, but it seems reasonable to conclude that in the light of all circumstances it would not be very likely. On the other hand, perhaps a city-state, much like the city-states of Shechem under Lab'aya or of Jerusalem under 'Abdi-Heba, would be feasible. Much in the David tradition would be compatible with that. The archaeology does not yet support this, but most do not see the existence of a citadel of the ruler in an otherwise small settlement at Jerusalem as being contradicted by the archaeology currently available. But a Jerusalem city-state is not the same as the 'united monarchy' of the Bible. It seems unlikely that David controlled anything beyond a limited territory centred on the southern hill country and Jerusalem. This might have overlapped with territory earlier controlled by Saul, which would lead to some of the biblical traditions that made David the usurper and successor of Saul.

When all is said and done, I fail to find a major distinction between the views of I. Finkelstein and A. Mazar. Mazar states:

It is certain that much of the biblical narrative concerning David and Solomon is mere fiction and embellishment written by later authors ... I would compare the potential achievements of David to those of an earlier hill country leader, namely Labayu, the *habiru* leader from Shechem ... David can be envisioned as a ruler similar to Labayu, except that he operated in a time free of intervention by the Egyptians or any other foreign power, and when the Canaanite cities were in decline. In such an environment, a talented and charismatic leader, politically astute, and in control of a small yet effective military power, may have taken hold of a large part of a small country like the Land of Israel and controlled diverse

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population groups under his regime from his stronghold in Jerusalem, which can be identified archaeologically. Such a regime does not necessitate a particularly large and populated capital city. David's Jerusalem can be compared to a medieval Burg, surrounded by a medium-sized town, and yet it could well be the centre of a meaningful polity. The only power that stood in David's way consisted of the Philistine cities, which, as archaeology tells us, were large and fortified urban centres during this time. Indeed the biblical historiographer excludes them from David's conquered territories. (Mazar forthcoming a)

Finkelstein states:

We may still be able to identify in them [the stories of David] the action of a local chieftain who moves with his gang to the south of Hebron, in the Judean Desert and in the Shephelah, far from the control of the central government in the highlands further to the north. David takes over Hebron, the second most important Iron Age town in the highlands of Judah and the centre of his theatre of operations, and then expands to the north and conquers Jerusalem, the traditional centre of government in the southern hill country. David, according to these stories, is a typical Apiru leader, who manages to establish a new dynasty in Jerusalem. (Finkelstein 2001: 107–8)

We now come to what many consider the central question: was there a 'united monarchy'? The question cannot be given a proper answer at this time, certainly not until the question of the LC is settled. But a partial answer can be given with some emphasis: *not as the Bible pictures it.*

CHAPTER 4

Iron IIB (900–720): Rise and Fall of the Northern Kingdom

For the first time since the Merneptah Inscription, it is in the reign of Omri that we finally begin to find extra-biblical data (apart from archaeology) with which to compare the picture given by the biblical text. Some are arguing that this is where the story of Israel begins – that Omri founded the first state in Palestine. In any case, the ninth and eighth centuries are dominated by the kingdom of Israel or northern kingdom.

4.1 Sources

4.1.1 Archaeology

One of the most significant areas of debate for this period is that of chronology. Because of the arguments of the LC hypothesis, some events conventionally dated to the tenth century would be in the ninth instead, if this scenario should turn out to be correct (see §1.2.3.4 for a discussion). For example, as noted earlier (§3.1.1), Finkelstein dates Hazor X to the time of Omri, or the ninth century.

The lower Galilee and the Jezreel Valley contained sites that were key to Israel in this period. Jezreel was the second main urban site in the region of Samaria, alongside the city of Samaria. There is evidence that, like Samaria, Jezreel was already settled in the tenth century BCE, before Omride rule (Zimhoni 1997; Ussishkin and Woodhead 1997: 68). The excavations there have suggested a new correlation with other contemporary sites at Megiddo and Taanach. Although it is not certain, the city gate seems to be of a six-chambered variety (Ussishkin and Woodhead 1997: 69; Ussishkin 2000:

248–50). This phase of Jezreel seems to be contemporary with Megiddo VA–IVB, with both destroyed at the same time; however, Ussishkin (2007) argues that Megiddo VA–IVB was constructed earlier, though not long before, and was taken by Shoshenq I (though only occupied by him, not destroyed). Based on the biblical narrative the main settlement Jezreel is usually ascribed to the Omrides (1 Kgs 18.45; 21.1), but it did not last long, perhaps being destroyed in Jehu’s revolt (ca. 842 BCE), but more likely by Hazael in the late ninth century (Ussishkin 2007; Na’aman 1997b: 125–7). (For N. Franklin’s views on Megiddo, Jezreel and Samaria, see below.)

Samaria is a critical site because its founding is directly associated with a historical event known from the Bible (1 Kgs 16.23–28). R. Tappy (1992, 2001) found, however, that the interpretation and dating methods used by K. Kenyon were flawed (§1.2.3.5). D. Ussishkin (2007) points out that Omri’s Palace and Inner Wall (Wall 161) belong to Kenyon’s Pottery Period I, and Casemate Wall, Ostraca Building and the building in the centre to Pottery Period II (though Franklin dates the Inner Wall to Period II). Structures in Pottery Period I continued in use in Pottery Period II. Since Ussishkin thinks the ‘floors’ are layers of natural soil, not laid floors, he argues that the structures of Periods I and II are all contemporary and built according to a single scheme and orientation. The dating of the acropolis is based primarily on the biblical evidence, with its construction assumed to be during the reigns of Omri and Ahab. That it was a sort of capital of the Omride kings is shown by the monumental architecture, as well as numerous Hebrew ostraca of an administrative nature and also Phoenician ivory carvings.

Building on Tappy’s insights, N. Franklin (2004; 2005; 2006; forthcoming a) has come up with a radically new interpretation of the remains from Samaria. The use of Samaria as a chronological anchor is based on the biblical narrative. Building Period 0 is the earliest, including primarily rock-cut cisterns and associated wine and oil preparation areas. Two rock-cut tombs have been identified as tombs of Omride kings below the Building Period I palace. In Franklin’s view Building Period I covered all of the Omride dynasty and part of Jehu’s. Building Period II consisted of a new regime during which time the summit of Samaria became a strictly administrative centre. A correlation between Megiddo VA–IVB and Samaria Building Period I is indicated by mason marks in situ at Megiddo and Samaria. Megiddo Palace 1723 and Samaria I Palace use a 0.45m Egyptian cubit. Both Megiddo VA–IVB and Samaria I, on the one hand, and Megiddo IVA and Samaria Building Period II, on the other, have architectural similarities. The ground plans of Megiddo IVA and Samaria II are laid out using the Assyrian cubit of 0.495 m. These considerations all show that Megiddo IVA and Samaria II

building methods were very different from the previous strata. Thus Samaria Building Period II is not a sequential addition to Building Period I. Only Samaria I is dated to the ninth century.

Finally, we come to the end of the northern kingdom. According to 2 Kgs 17.5-6, 23-24, the Assyrians besieged Samaria for three years before taking it and deporting all the inhabitants of the kingdom to Mesopotamia, replacing them with peoples from Mesopotamia. From an archaeological point of view there are some problems with this scenario. There is no burn layer or other evidence of destruction for Samaria, raising questions about the 2 Kgs 17.5-6 statement that Samaria was besieged for three years. As for the displacement of peoples, A. Zertal (1989; 1990: 12-14) has analysed the finds of a wedge-shaped decorated bowl from a confined area in the old territory of Manasseh. He argues that this pottery feature shows the settlement area of those Mesopotamians brought into Samaria in the late eighth century. This region, with Tell el-Far'ah (north) at its centre but with Samaria on its edge, is only a small part of the province of Samaria. If Zertal's analysis is accepted, the entire former kingdom of Samaria was not involved nor was the entire population deported. Zertal (1990: 82-3) estimates that the imported population was no more than a few thousand, and deportation affected not more than 10 per cent of the Israelite population, the vast majority of which continued to live in Samaria. How all this might relate to the text is discussed below (§4.2.4).

Almost all the regions of Ephraim were intensively settled (Finkelstein 1988-89: 151-4). Compared with Iron I, the population had shifted west, with some of the large sites in the east abandoned: Shiloh, 'Ai and Khirbit Raddana. Part of the reason seems to be the economic importance of horticulture, for which the slopes and foothills were better suited. Sufficient grain was grown in some regions, apparently, to allow the western regions to concentrate on the important wine and oil production. In the southern central range of the Ephraimite hills some sites were abandoned and a fall in the population generally in this region is probably to be explained by border conflicts and tensions between the kingdom of Israel and the kingdom of Judah. The population seems to have reached its peak in the mid-eighth century.

The Shephelah only began to be populated in Iron IIA. In Iron IIB we witness an impressive growth of settlement numbers in the whole area of Judah. This included strata IV-III of Lachish and the parallel strata in other sites. Y. Dagan (2004: 2680-1) concluded that in the Shephelah in Iron IIA and IIB the sites developed slowly as the process of Judaeian settlement began, with 731 sites in Iron IIB. The prosperity of the period reached its zenith in the eighth century. The Shephelah (with about 2500 settled dunams)

and Benjamin overshadowed the Judaeen highlands. This ends, of course, with the massive destruction by Sennacherib in all areas south and west of Jerusalem. Lachish III is an important chronological indicator since it seems to have been destroyed by Sennacherib in 701 BCE.

As for the Judaeen hills, some of the differences in interpretation were discussed above (§3.1.1). A. Ofer (2001; 1994) had argued that from the mid-eleventh to the eighth century the population nearly doubled in each century. In the ninth century this included 66 settlements covering 50 hectares; in the eighth, 88 settlements covering 90 hectares, which would mean a population of about 22,000–23,000. Any discussion of Jerusalem has to take account of the debate over the state of the city in the tenth and ninth centuries and the question of the united monarchy, which is discussed in the previous chapter (§3.1.1). Regardless of the answers, all agree that the archaeology of Jerusalem in the eighth century shows a significant city and the capital of a fully fledged state. Excavations on the Ophel show the earliest buildings there date only from the ninth century. E. Mazar had argued that the fortified complex of this area south of the Temple Mount had been constructed as early as the ninth century, but they more likely date between the eighth and the early sixth centuries BCE (Killebrew 2003: 336). E. A. Knauf (2000) argues that the centre of the Davidic city has not been found because it would have been the area north of the Ophel, the area of the Temple Mount. Although it is not possible to test this hypothesis now, that section of the hill was a militarily strategic area and would have had to have been incorporated into any settlement on the southeastern hill. The argument that the MB wall was used as a city wall in LB, Iron I, and Iron IIA and IIB has no support; Jerusalem lacked a fortification wall until the mid-eighth century (Killebrew 2003: 334). From the late eighth century there is plenty of evidence for Jerusalem as a major urban centre.

One of the main controversies over a number of decades has to do with the size of Jerusalem in the last part of the Judahite monarchy (Geva 2003). Researchers have been divided with regard to when the fortified area expanded out of the southeastern ridge to include the southwestern hill. The ‘minimalist’ view was prevalent in the mid-twentieth century, while the ‘maximalists’ disagreed among themselves as to when expansion took place. Minimalists pointed to lack of archaeological finds on the southwestern hill, because the excavations of Kenyon and Tushingham on the eastern slope of the southwestern hill found nothing from the end of the monarchy. However, N. Avigad conducted excavations in the Jewish Quarter in 1969–82 (Avigad and Geva 2000). A significant find was a massive wall that Avigad identified with the ‘Broad Wall’ of Neh. 3.8. This settlement on the southwestern hill began in the mid-eighth century BCE. It was natural to associate the

Broad Wall and other building activity with the reign of Hezekiah. This wall went out of use not long after it was built, and a new wall was constructed north of it in the seventh century (§5.1.1). New suburbs seem to have been established: *mišneh* (2 Kgs 22.14; Zeph. 1.10: a new residential quarter on the southwestern hill) and the *maktēš* (Zeph. 1.11: apparently the name of the central valley). Jerusalem experienced unprecedented growth, reaching 60 hectares (the southwestern hill 45). Geva gives a conservative estimate of the population in the eighth century as 6,000–7,000 (in contrast to Broshi's 20,000 [2001]). The indications are that the southwestern hill was sparsely settled, but there was also an expansion to the north. The earliest tombs to the west of the city in Ketef Hinnom and of Mamilla are also dated to the eighth century (Reich and Shukron 2003: 211).

Recent excavations near the Gihon Spring have yielded some results that have been dated to the eighth century BCE (Reich, Shukron and Lerna forthcoming). A house was built in an abandoned pool, with the floor level created by fill presumably taken from the immediately surrounding area. The pottery from this fill was different from other late Iron pottery found in the area in a decade of digging: this pottery was more like Lachish IV or at least the early eighth century BCE and possibly the late ninth (a final verdict awaits completion of the sifting process). Four scaraboid seals, four scarabs and fragments of approximately 150 clay bullae bearing impressions were found (probably the remains from documents that had been unsealed). All are anepigraphic, but motifs include pseudo-hieroglyphs, proto-Ionic capitals, winged suns, winged griffins, and an almost complete Phoenician ship. Overall, the collection points to the Phoenician realm. The large number of bullae and the seals, plus a 15-holed plaque (apparently a scribal device with a time-keeping function), point to a nearby administrative centre in the late ninth century. It might be that this shows the introduction of record-keeping and bureaucracy into Judah directly or indirectly through the services of the Omride kingdom (with its Phoenician connections).

In the Beersheba valley, it is now accepted that Arad XI and Beersheba V (with the first Iron Age fortifications in Judah) must be in the ninth century (Herzog 2001; 2002: 58–72). Herzog associates strata XI–VI with the Iron II period. Much controversy has centred around the temple on the site. The building of the temple's initial stage was associated with stratum X, which now needs to be dated to the eighth century; the second phase, with stratum IX (the stela and incense altars were used with this phase); the temple no longer existed in stratum VIII. Thus, the temple was built in the eighth century and dismantled about 715 BCE, having lasted no more than about 40 years (see further at §5.1.1). Much of strata VII and VI were removed by Hellenistic and Roman builders.

Considerable changes took place in the Beersheba valley and region during the ninth and eighth centuries (Herzog 2002: 94–9). The climate worsened at the beginning of the ninth century, which made cultivation much more difficult in this marginal region. This seems to have coincided with the establishment of a regional administrative–military centre in place of the agricultural settlements, taking in Arad XI, Beersheba V, Tel Masos I and Tel Malhata. The population of the area nevertheless appears to have remained much the same, no more than about 1,000. A major cultural shift took place between the ninth and eighth centuries, with a completely different pottery assemblage for Arad X–XIII and Beersheba III–II. The massive fortifications of Beersheba IV and Arad XI were razed to the ground, followed by a much weaker replacement build (Herzog and Singer-Avitz 2004: 230). If the initial destruction was caused by enemy action, the replacement defences would have attempted to be at least as strong as those that had stood there before. Hence, a natural disaster such as an earthquake seems to be the cause. In the eighth century Tel ‘Ira VII was resettled, and fortresses were built at Kadash Barnea and Tell el-Kheleifeh. The settled area was twice that of the ninth century. There is also evidence for Beersheba as a ‘gateway community’ (supply station) for the trade route(s) from Arabia at this time (Singer-Avitz 1999).

The archaeology does confirm some textual hints at outside influences (Phoenicia, Syria, Neo-Hittite) on the northern kingdom (Barkay 1992: 306, 335–8). Most basic was Cypro-Phoenician pottery (Barkay 1992: 338). The ivories found in Samaria and elsewhere tend to be seen as showing Phoenician influence (Barkay 1992: 320–3). Many of the monumental and administrative buildings in the north appeared to be influenced by *bit-hilani* architecture known earlier from Syria (Reich 1992: 202–6; Barkay 1992: 310). Ashlar masonry (including the paving of square areas) and proto-Aeolic capitals are also often thought to be Phoenician imports (Stern 1992: 302–5).

4.1.2 Hebrew Inscriptions

The number of Hebrew inscriptions for this period is surprisingly small. This fact is one of the supports used by those who argue that writing only began to be used extensively in the latter part of the monarchy (§3.2.5). What we have are mostly ostraca (Renz 1995: I, 40–144). The main texts – those with legible text of any length – are the Kuntillet ‘Ajrud inscriptions (Gogel 1998: #6.1.14) and the Samaritan ostraca (Gogel 1998: #6.1.21; TSSI 1; Lemaire 1977). The former are mainly of religious interest and are discussed below

in that context (§4.2.5). The Samarian ostraca are usually dated to the early eighth century and explained as administrative documents relating to wine and oil brought into the city from outside (Kaufman 1992). H. M. Niemann (2007) has recently explained them differently: they document attempts by Jehoash and Jeroboam II to integrate the traditional tribal links into their personal power network. It was the practice to have elite members of the clans reside as ‘honoured guests’ in Samaria for shorter or longer periods of time, stabilizing political links between the Samarian king (warlord) and the surrounding clans. Wine and oil were sent from their local constituents to the clan members residing in Samaria, and the ostraca are to be explained as receipts for these goods. The ostraca are an important source of data on names of people in ancient Israel.

4.1.3 Aramaic Inscriptions

4.1.3.1 Tel Dan

Perhaps one of the most interesting texts is the one recently found at Tel Dan, which already has a considerable bibliography (Athas 2003; Kottsieper 2007). The following is my reading of the first fragment found in 1993 (Biran and Naveh 1993) with a minimum of reconstruction:

] my father went up [
my father lay down (died?). He went to [Is-]
rael earlier in the land. My father [(or ‘in the land of my father’)
I – Hadad went before me [
x my king. And I killed of [them char-]
ot and thousands (or 2000) of riders [
king of Israel. And I kill[ed
xx ‘house of David’ (*bytdwd*). And I set [
xx the land. They x[
another, and xxxx [ki-
ng over Is[rael
siege over {

The second fragment (actually two fragments that fit together) does not clearly join onto the first, and the reconstruction based on putting the two together strikes me as purely speculative (cf. Athas 2003: 175–91). I read the second fragment as follows, with little hypothetical reconstruction:

] and cut [
] battle/fought against xx [
]x and went up the king x [
] and Hadad made king [
] I went up from Sheva'/seven [
 seven]ty tied/harnessed x[
]rm son [
]yahu son [

This inscription has been subject to a number of interpretations, some of which are quite compelling, but they rely generally on the reconstruction of the original editors. However, it does seem to me that in the last two lines above the restoration of 'J(eh)oram' is virtually certain, and of 'Ahaziah' quite reasonable. If so, this favours assigning the inscription to Hazael and the interpretations that follow from it. Finally, there have been some isolated doubts about the authenticity of one or more of the fragments, but these have not been convincing (Athas 2003: 22–35, 70–2).

4.1.3.2 Melqart Inscription

This is dated to the ninth or early eighth century BCE. A number of the older readings must now be discarded in the light of recent study. It mentions 'Bir-Ha[dad], son of 'Attar-hamek, king of Aram' (Pitard 1988: 272; cf. Puech 1992).

4.1.3.3 Zakkur Inscription

The Zakkur Inscription is dated to about 800 BCE.

The monument which Zakkur, king of Hamath and Lu'ash, set up for El-wer [in Apish] ... Bar-Hadad, son of Hazael, king of Aram, united against me s[even]teen kings: Bar-Hadad and his army, Bar-Gush and his army, the king of Que and his army, the king of 'Amuq and his army, the king of Gurgum and his army, the king of Sam'al and his army, the king of Meliz and his army [] seven[teen], they and their armies. [CoS II,155 (text 2.35)]

4.1.4 Mesha Stela

The importance of the Mesha Stela or the Moabite stone has long been recognized. The exact date of the inscription is difficult because it depends largely on assumptions about the historical context (Lemaire 1994a; 1994b; Na'aman 2007). For the purposes of this exercise, we cannot take that context for granted; however, the text is likely to date from the ninth or eighth century BCE. Only lines 1–9 are given here:

I am Mesha, the son of Kemosh[-yatti], the king of Moab, the Dibonite. My father was king over Moab for thirty years, and I was king after my father. And I made this high-place for Kemosh in Karchoh, [...] because he has delivered me from all kings(?), and because he has made me look down on all my enemies. Omri was the king of Israel, and he oppressed Moab for many days, for Kemosh was angry with his land. And his son succeeded him, and he said – he too – ‘I will oppress Moab!’ In my days did he say [so], but I looked down on him and on his house, and Israel has gone to ruin, yes, it has gone to ruin for ever! And Omri had taken possession of the whole la[n]d of Medeba, and he lived there (in) his days and half the days of his son, forty years, but Kemosh [resto]red it in my days. And I built Baal Meon, and I made in it a water reservoir, and I built Kiriathaim. [CoS II, 137 (text 2.23)]

The historical implications are discussed in some detail below (§4.2.2).

4.1.5 Assyrian Sources

In the Kurkh Monolith which details Shalmaneser III's campaign in his sixth year (853 BCE), he describes the force that met him after he had worked his way as far as Qarqar on the Orontes in northern Syria:

1200 chariots, 1200 cavalry, 20,000 footsoldiers of Adad-idri ‘of Aram-Damascus ([šá KUR]-ANŠE-šú)’; 700 chariots, 700 cavalry, 10,000 footsoldiers of Irhuleni, ‘the Hamathite (KUR *A-mat-a-a*)’; 2,000 chariots [2 lim GIŠ.GIGIR.MEŠ], 10,000 footsoldiers of Ahab (^m*A-ha-ab-bu*) ‘the Israelite (KUR *Sir’-la-a-a*)’ ... these 12 kings, he brought as his allies. They came against me to [wage] war and fight. In the exalted might which Ashur my lord gave me (and) with the strong weapons which Nergal, who goes before me, presented to me, I fought with them. I defeated them from Qarqar to Gilzau. I slew 14,000 of their soldiers with the weapons (and) rained, like the god Adad, the destructive flood upon them. [Yamada 2000: 156–7, 376]

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The Baghdad Text (Grayson 1996: 32–41 [A.0.102.6]) and the Calah Annals (Grayson 1996: 42–8 [A.0.102.8]) indicate that the coalition continued to oppose the Assyrians successfully for many years – Shalmaneser’s 10th year: opposed by Hadadezer of Damascus, Irḫulēnu of Hamath, together with a coalition of twelve kings; 11th year: opposed by Hadadezer, Irḫulēnu, together with a coalition of twelve kings; 14th year: Shalmaneser has an army of 120,000; opposed by Hadadezer, Irḫulēnu, together with a coalition of twelve kings. However, in his 18th year, the situation is different (Aššur Basalt Statue):

I defeated Adad-idri of Damascus with 12 kings, his helpers, and laid down 29,000 of his brave fighters like reeds. The remainder of his army, I cast into the Orontes river. They fled to save their life. Adad-idri died. Hazael, son of a nobody [DUMU *la ma-ma-na*], took the throne. He mustered his large army and came against me to wage war. I fought with him and defeated him (and) took off the wall of his camp. Hazael fled to save his own life. I pursued (him) as far as Damascus, his royal city. [Yamada 2000: 188–9; cf. also Grayson 1996: 118 (A.0.102.40: i 14–35); *ANET*: 280]

The Kurba’il Statue gives some similar information but also adds data not in the other inscriptions:

In my eighteenth regnal year, I crossed the Euphrates for the sixteenth time, Hazael of Damascus trusted in the massed might of his troops; and he mustered his army in great numbers ... I decisively defeated him. I felled with the sword 16,000 of his men-of-arms. I took away from him 1,121 of his chariots, 470 of his cavalry, together with his camp. In order to save his life he ran away. I pursued after him. I confined him in Damascus, his royal city... At that time I received the tribute of the Tyrians, the Sidonians, and Jehu (*la-ú-a*), (the man) of Bit-Ḫumrî (Omri). [CoS II, 268; cf. also Grayson 1996: 60 (A.0.102.12: 21–30)]

On the designation of Jehu as DUMU *ḫu-um-ri-i*, often translated as ‘son of Omri’, see below (4.2.1).

Shalmaneser’s successor, Shamshi-Adad V (823–811), had to deal with a rebellion that had begun under Shalmaneser III, and the west of the empire was more or less abandoned during his reign. As will be seen below, this left the opportunity for Damascus to have a free hand against its neighbours.

The next king, Adad-Narari III (810–783), had a long reign with important campaigns to both the east and the west. (It is widely thought that Šamši-Adad and his mother formed the basis of the Nimrod and Semiramis legend [Grabbe 2003b: 122–5].) He renewed campaigns to Syria and the

Mediterranean (ca. 805–796), though the number of campaigns is uncertain. According to the el-Rimah Inscription,

In one year I subdued the entire lands Amurru (and) Ḫatti. I imposed upon them tax (and) tribute forever. I (text ‘he’) received 2000 talents of silver, 1000 talents of copper, 2000 talents of iron, 3000 linen garments with multi-coloured trim – the tribute from Mari, the Damascene. I (text ‘he’) received the tribute of Joash (Iu’asu), the Samaritan, (and) of the people of Tyre (and) Sidon. [Grayson 1996: 211 (A.O.104.7.4-8); cf. Page 1968: 143; ANET: 281–82]

‘Iu’-a-su of Samaria (*Sa-me-ri-na-a-a*) [var. land of Omri (*Ḫu-um-ri-i*)]’ is almost certainly Joash of Israel (Weippert 1978, which corrects the view expressed by McCarter [1974] that *Iuas* should be taken as Jehu). More difficult is Mari of Damascus. *Mari* can be simply the Aramaic title, ‘my lord’ rather than a personal name. It has been suggested that the Assyrians referred to the Aramaean leader by this term because they were uncertain as to who he was (Lipiński 2000: 390–1). The Arslan Tash ivory might suggest that this was Hazael: ‘[]xx ‘*m*’ to our lord (*mr’n*) Hazael in the year [of the tak]ing of Ḫ[.]’ (Puech 1981: 544–62). A similar message is found on the Hazael booty inscriptions which seems to read something along the line of the following: ‘That which Hadad (?) gave to our lord (*mr’n*) Hazael from Umeq in the year that our lord crossed over the river’ (Eph’al and Naveh 1989: 192–200; also Lipiński 2000: 388). The name in the Eretria horse-blinder inscription seems to have a *b* as the last letter of the name, though this could be a scribal idiosyncrasy. Others, however, would identify Mari with Bar-Hadad son of Hazael (Pitard 1987: 165–6; Millard and Tadmor 1973: 63 n. 22; Kuan 1995: 81 n. 46).

The next 40 years (782–745 BCE) are rather obscure in Assyrian history. The period seems to be characterized by weak rulers and strong officials. Military campaigns were not always led by the king, and there were non-campaign years. It was basically a time of contracting borders and major problems with Urartu, with the Assyrians losing their hold on the west. However, the *tartanu* (vice-regent) Šamši-Ilu was an important figure in the region. The Maraş Museum inscription describes how he took tribute from Damascus in the time of Shalmaneser IV (782–773 BCE):

When Šamši-ilu, the field marshal, marched to Damascus, the tribute of Ḫadiiāni, the Damascene – silver, gold, copper, his royal bed, his royal couch, his daughter with her extensive dowry, the property of his palace without number – I received from him (Ḫadiiāni). On my return (from Damascus) I gave this boundary stone to Ušpilulume, king of the Kummuhites. [Grayson 1996: 240 (A.O.105.1.4-13)]

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Tiglath-pileser III (744–727 BCE) was probably a usurper. Little building activity seems to have taken place because of the extensive campaigns. The three-fold system of vassalage was perfected under Tiglath-pileser. Campaigns to the west took place in 743, 738 and through the period 734–732. Annals 19* (lines 9–11) mention the rebellion of Azriyau in 738 BCE:

19 districts of Hamath together with the cities of their environs, which are on the seacoast of the west, which in rebellion were seized for Azriyau, I annexed to Assyria. I placed two of my eunuchs over them as governors. (Tadmor 1994: 62–3).

This Azriyau was early identified with Azariah (=Uzziah) of Judah (2 Kgs 15.1–7), partly based on the mistaken assumption that another Assyrian inscription referred to ‘Azriyau of Yaudi’ (Tadmor 1961; 1994: 273–4). This identification has been largely abandoned, partly because Azriyau’s country is not known and partly because the coalition led by Azriyau seems to be made up of states in the area of central and northern Syria (Kuan 1995: 149 n. 57).

Tiglath-pileser III’s tribute list in the Calah Annals 13* and 14* tells of the tribute collected about 738–737 BCE (Annal 13*, line 10, to 14*, line 5):

The tribute of Kushtashpi of Kummuh, Rezin (Rahianu) of Damascus, Menahem of Samaria, Hiram of Tyre, Sibittibi’il of Byblos, Urikki of Que, Pisiris of Carchemish, Eni-il of Hamath, Panammu of Sam’al ... I received. (Tadmor 1994: 69–71)

Summary Inscription 4 (lines 7–20) describes the final destruction of Damascus and the change of king in Israel (ca. 732 BCE):

The ent[ire] wide land of [Bit-Haza’i]li (Aram) I annexed to Assyria. [I plac]ed [x eunuchs over them] as governors. Hanunu of Gaza, [who] fle[d before] my weapons, (and) escaped [to] Egypt – Gaza [... his royal city, I conquered/entered] his property (and) his gods [I despoiled/seized. A (statue) bearing the image of the gods my lo]rds and my (own) royal image [out of gold I fashioned.] In the palace [of Gaza] I set (it) up (and) I counted it among the gods of their land; [their ...] I established. As for [him (i.e. Hanunu), the fear of my majesty] overwhelmed him and like a bird he flew (back) [from Egypt] [...] I returned him to his position and his [... I turned into an Assyrian emporium. Gold,] silver, multi-coloured garments, linen garments, large [horses,] ... [...] I received. The land of Bit-Humria (Israel), [... its] ‘auxiliary army’, [...] all of its people, [...] I carried off [to] Assyria. Peqah, their king [I/they killed] and I installed Hoshea [as king] over them. 10 talents of gold, x talents of silver, [with] their [property] I received

from them and [to Assyria I car]ried them. As for Samsi, the queen of the Arabs, at Mount Saqurri I de[feated 9,400] (of her people). (Tadmor 1994: 138–41)

The actual siege of Damascus is described in Tiglath-pileser III's Calah Annals 23. Summary Inscription 7 also tells us that Jehoahaz king of Judah (^m*Ia-ú-ba-zi* ^{kur}*Ia-ú-da-a+a*) paid tribute about this time (reverse, lines 10'–12'):

10' [Ma]tanbi'il of Arvad, Sanipu of Ammon, Salamanu of Moab, [.....]
11' [Mi]tinti of Ashkelon, Jehoahaz of Judah, Qaushmalaka of Edom, Muš...
[...of.....]
12' (and) Hanunu of Gaza... (Tadmor 1994: 170–1)

The fall of Samaria is actually claimed by two kings. First is Shalmaneser V (726–722 BCE) who apparently as crown prince had acted as chief administrator while his father was campaigning. Little is known of him apart from his siege of Samaria, as recorded in *Babylonian Chronicle 1* (i 27–31):

On the twenty-fifth day of the month Tebet Shalmaneser (V) ascended the throne in Assyria <and Akkad>. He ravaged *Samaria*. The fifth year: Shalmaneser (V) died in the month Tebet. For five years Shalmaneser (V) ruled Akkad and Assyria. On the twelfth day of the month Tebet Sargon (II) ascended the throne in Assyria. (Grayson 1975: 73)

The editor has put '*Samaria*' in italics, but it seems to be agreed by consensus that the ^{uru}*Šá-mal/ba-ra-'in* of the text should be so read. However, Sargon II claims in his *Annals* to have conquered Samaria:

In the begin[ning of my reign when I took (my) seat on the royal throne and was crowned with a lordly crown, the Sama]rians, [who agreed with (another) hostile king not to continue their slavery and not to deliver tribute and who started hostility in the strength of.....who b]ring about my triumph, [I fought] w[ith them and completed their defeat. 27280 (or 27290) people, who lived therein, with their chariots, I] carried off (as) spoil. 50 chariots (for) my royal bodyguard [I mustered from among them and the rest of them I settled in the midst of Assyria (= Assyria proper). The city of Samaria I re]settled and made it greater than before. People of the lands conquere[d by my own hand (= by myself) I brought there. My courtier I placed over them as governor and duties and] tax I imposed upon them as on Assyrians. (Tadmor 1958b: 34)

The Nimrud Prism, which is better preserved, is parallel and gives similar

information (Tadmor 1958b: 34).

4.1.6 Phoenician History of *Menander of Ephesus*

Unfortunately, we have little historical material on Phoenicia, and what little there is comes mainly from classical writers. Menander of Ephesus is alleged to have written a *Phoenician History* using ancient records. A relevant passage for purposes of this paper is the following:

This rainless time is also mentioned by Menander in his account of the acts of Ithōbalos, the king of Tyre, in these words: 'There was a drought in his reign, which lasted from the month of Hyperberetaios until the month of Hyperberetaios in the following year. But he made supplication to the gods, whereupon a heavy thunderstorm broke out. He it was who founded the city of Botrys in Phoenicia, and Auza in Libya.' This, then, is what Menander wrote, referring to the drought which came in Ahab's reign, for it was in his time that Ithōbalos was king of Tyre. (Quoted by Josephus, *Ant.* 8.13.2 §324.)

There are many questions. What was his source? Was it a genuine Phoenician one or simply another Greek writer? Was the source (if there was one) in Greek or Phoenician? Who made the connection with the father of Jezebel, Josephus or his source?

A Phoenician sarcophagus inscription from Byblos may have some useful information: 'The sarcophagus that 'Ittoba'l, son of 'Ahirom, the king of Byblos, made for 'Ahirom, his father, when he placed him in eternity' (CoS II, 181 [text 2.55]). Exactly when to date this can only be guessed. The name of the son is the same as the father of Jezebel, but was this only a common name for Phoenician kings? This individual is king of Byblos, not Tyre, though Sidon may have been a general term for 'Phoenician' by outside writers at the time.

4.1.7 Biblical Text

Unlike chapters 2 and 3 where large amounts of text are covered, only a few chapters are involved here; therefore, a more detailed outline of contents is presented. Because the books of Chronicles focus on Judah, parallels in 2 Chronicles to the Ahab story are only sporadic. They are listed where they occur, but this is not very often.

4.1.7.1 1 Kings 16.15–2 Kings 17.41 (2 Chronicles 18–28): Outline of the Contents

- 1 Kgs 16.15-28: The reign of Omri.
 - 16.15-20: Zimri reigns seven days in Tirzeh after a coup but is defeated by the army commander Omri and commits suicide.
 - 16.21-22: The people are split between Omri and Tibni, but Omri defeats and kills the latter.
 - 16.23-24: Omri reigns 12 years, buys a hill and founds the city of Samaria.
 - 16.25-28: Omri's reign is given a theological summary without additional information.
- 1 Kgs 16.29-34: A theologizing summary of Ahab's reign.
 - 16.29-33: Ahab reigns 22 years, marries Jezebel, and worships Baal, building a temple to Baal in Samaria.
 - 16.34: Hiel builds Jericho at the expense of his first born and youngest sons.
- 1 Kgs 17–19: Various tales centring on Elijah the prophet/man of God.
 - 17.1-6: Elijah declares a famine to Ahab, then hides where he is fed by ravens.
 - 17.7-16: Elijah lives with a widow where they are fed miraculously.
 - 17.17-24: Elijah heals/raises to life the son of the widow.
 - 18.1-46: Elijah's contest with the prophets of Baal and the end of the drought.
 - 19.1-14: Elijah flees to Horeb from the wrath of Jezebel.
 - 19.15-18: Elijah is sent to anoint Hazael, Jehu and Elisha.
 - 19.19-21: Elisha leaves his farming to follow Elijah.
- 1 Kgs 20: Ahab and the attack of the Aramaeans.
 - 20.1-21: Ben-Hadad threatens Ahab but is defeated by Israel at the word of a prophet.
 - 20.22-30: The prophet warns of a second invasion, but the Aramaeans are defeated at the word of a man of God.
 - 20.31-34: Ben-Hadad surrenders to Ahab who spares his life; Ben-Hadad promises to return the towns taken from Ahab's father by his father, and to let Israel set up bazaars in Damascus as his father did in Samaria.
 - 20.35-43: A prophet has himself wounded and appears to Ahab as a sign of his error in releasing Ben-Hadad.
- 1 Kgs 21: Ahab and Naboth's vineyard.
 - 21.1-16: Ahab takes Naboth's vineyard.
 - 21.17-26: Elijah prophesies against Ahab and his house.

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- 21.27-29: Ahab repents and the prophecy against him is postponed to his son's time.
- 1 Kgs 22//2 Chron. 18: Story of Ahab's death.
- 22.1-5//18.1-4: Ahab asks Jehoshaphat for help in retaking Ramoth-gilead.
- 22.6-28//18.5-27: Prophets are consulted, including Micaiah who prophesies defeat for Ahab.
- 22.29-38//18.28-34: Ahab is killed in battle, fulfilling Yhwh's word.
- 22.39-40: Summary of Ahab's reign, including the 'ivory house' that he built.
- 22.41-51//20.31-21.3: Summary of Jehoshaphat's reign, including his 'righteousness' and his failed plan to send ships of Tarshish to Ophir for gold.
- 22.52-54: Ahaziah son of Ahab becomes king and reigns two years.
- 2 Kgs 1: Ahaziah dies at the word of Elijah for enquiring of Baal-Zebub (most of the chapter concerns the efforts to get Elijah to come and visit the king on his sick bed).
- 2 Kgs 2: Elijah is taken to heaven and Elisha assumes his mantle.
- 2 Kgs 3: Coalition of kings attack Moab.
- 3.1-3: Jehoram becomes king over Israel.
- 3.4-9: Mesha the king of Moab rebels against Jehoram who assembles a coalition of Israel, Judah and Edom.
- 3.9-20: The kings are delivered from lack of water by Elisha.
- 3.21-27: Moab is defeated but Israel withdraws after Mesha sacrifices the crown prince.
- 2 Kgs 4.1-8.15: tales about Elisha.
- 4.1-44: Various tales of Elisha.
- 5.1-27: Elisha heals Naaman the Aramaean commander.
- 6.1-7: Elisha makes an axehead float.
- 6.8-23: Elisha delivers the Aramaean army into the king of Israel's hand, and the Aramaeans stop invading the land of Israel.
- 6.24-7.20: Ben-Hadad besieges Samaria, but the Aramaeans flee and the city is delivered through Elisha.
- 8.1-6: Elisha helps the woman whose son he had revived.
- 8.7-15: Elisha prophesies that Hazael will replace the ill Ben-Hadad, and the former assassinates the latter.
- 2 Kgs 8.16-24//2 Chron. 21.4-11, 21.20-22.1: Summary of the reign of Joram son of Jehoshaphat over Judah: he marries Athaliah, daughter of Ahab; Edom gains its independence at this time.
- 2 Kgs 8.25-29//2 Chron. 22.2-6: Joram of Judah dies and his son Ahaziah reigns; Joram of Israel fights alongside Ahaziah against Hazael and the

- Aramaeans at Ramoth-Gilead and is wounded.
- 2 Kgs 9–10: Jehu stages a coup and takes the throne.
- 9.1-14: Elisha sends a disciple to anoint Jehu king in Ramoth-Gilead.
- 9.15-29//22.7-9: Jehu kills Joram of Israel and Ahaziah of Judah.
- 9.30-37: Jehu kills Jezebel.
- 10.1-11: Jehu causes Ahab's offspring to be killed.
- 10.12-14: Relatives of Ahaziah are killed.
- 10.15-17: Jehu takes Jehonadab the Rechabite to witness his slaughter of the rest of Ahab's house.
- 10.18-28: Jehu slaughters the worshippers of Baal and removes Baal worship from Israel.
- 10.29-31: Yet Jehu does not follow the Torah or remove worship at Bethel and Dan.
- 10.32-33: Hazael takes Israelite land east of the Jordan.
- 10.34-36: Summary of Jehu's reign.
- 2 Kgs 11–12//2 Chron. 22.10-24.14: Coup of Joash and his reign over Judah.
- 11.1-20//22.10–23.21: Execution of Athaliah and the installation of Joash on the throne in Jerusalem.
- 12.1-17//24.1-14: Joash has money collected to repair Yhwh's house.
- 12.18-19: Hazael of Aram comes against Jerusalem, but Joash buys him off.
- 12.20-22: Joash's death and summary of his reign.
- 2 Kgs 13.1-9: Reign of Jehoahaz son of Jehu.
- 13.1-2: Summary of Jehoahaz's reign.
- 13.3: Israel afflicted by Hazael and Ben-Hadad of Aram.
- 13.4-6: Israel gains freedom from Aram despite remaining in the sins of Jeroboam.
- 13.7: Jehoahaz had been left with only a small military force.
- 13.8-9: Jehoahaz's death and summary of his reign.
- 2 Kgs 13.10-25: Reign of Jehoash of Israel.
- 13.10-13: Summary of Jehoash's reign.
- 13.14-19: Elisha on his death bed offers to Jehoash a sign of defeat of Aram.
- 13.20-21: Death of Elisha and resurrection of corpse by his bones.
- 13.22-25: Jehoash son of Jehoahaz begins to recover territory from Ben-Hadad.
- 2 Kgs 14.1-22//2 Chron. 25.1–26.2: Reign of Amaziah of Judah.
- 14.1-6//25.1-4: How he was righteous (though he did not remove the country shrines).
- 14.7//25.5-16: Defeat of the Edomites (version in 2 Chronicles is greatly

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- expanded, with anecdotes about prophets).
- 14.8-14//25.17-24: Challenges Jehoash of Israel to battle and is defeated and Jerusalem sacked.
- 14.15-16: Death of Jehoash of Israel and summary of his reign.
- 14.17-20//25.25-28: Amaziah is assassinated in a conspiracy; summary of his reign.
- 14.21-22//26.1-2: Azariah (Uzziah) made king of Judah; restores Elath (Elath) to Judah.
- 2 Kgs 14.23-29: Jeroboam becomes king of Israel; restores territory from Lebo-Hamath to the sea of Arabah, including Damascus and Hamath, according to the word of Jonah.
- 2 Kgs 15.1-7//26.3-23: Reign of Azariah (Uzziah) who is righteous but becomes a leper.
- 2 Kgs 15.8-12: Zechariah becomes king of Israel but is assassinated, as last of Jehu dynasty.
- 2 Kgs 15.13-15: Shallum reigns one month.
- 2 Kgs 15.16-22: Menahem is king over Israel; Pul of Assyria invades and Menahem gives him 10,000 talents.
- 2 Kgs 15.23-26: Reign of Pekahiah of Israel.
- 2 Kgs 15.27-31: Reign of Pekah of Israel; Tiglath-pileser invades and takes northern Israel.
- 2 Kgs 15.32-38//2 Chron. 27.1-9: Reign of Jotham of Judah; attacked by Rezin of Aram and Pekah.
- 2 Kgs 16//2 Chron. 28: Reign of Ahaz of Judah.
- 16.1-4//28.1-4: Theologizing summary of Ahaz's reign.
- 16.5-9//28.5-19: Syro-Ephraimite war.
- 16.10-16//28.20-27: Ahaz makes an altar like the one of Tiglath-pileser in Damascus.
- 2 Kgs 17: Fall of Samaria.

4.1.7.2 Analysis

Any modern historians worth their salt would immediately recognize that not all the material in the biblical text is of the same quality. First, leaving aside for the moment the long tradition of source and redaction criticism among biblical scholars, a careful reading of this lengthy stretch of text will still detect that along with narratives giving the impression of describing unfolding events are other sorts of material that look distinctly legendary. This especially applies to stories in which the chief protagonist is a prophet. Thus, whereas the reign of Omri is described succinctly and with the *prima*

facie appearance of factuality, the reign of Ahab is dominated by stories about the prophets Elijah and Elisha. A second point that would be obvious to a modern historian is that here and there are theological summaries and that the overall perspective is one of theological judgement (e.g., the success and failure of kings is according to whether they have been 'righteous' or 'wicked' according to the religious code of the writer). Moral judgement on rulers is not in itself unusual in ancient writings, including the Greek historians. Nevertheless, it would be noticeable to a modern historian and would need to be taken account of (see further Grabbe 2001b).

In fact, if we look carefully at the material stretching from 1 Kings 16 to 2 Kings 17, we see few passages of any length that seem to be straightforward narratives. On the contrary, the whole section is dominated by prophetic stories or theological interests. For example, an entire chapter is devoted to the reign of Joash of Judah (2 Kings 12), yet most of the space concerns how money was collected for the repair of the house of Yhwh. There is little from 1 Kings 17 to 2 Kings 9 that does not relate to Elijah and Elisha. Much of this long section of 15 chapters is a series of prophetic stories and anecdotes. Some of these relate to national events, but a good deal is purely focused on the doings of the prophet.

We have one new development, however, which is very important for the question of historicity. Beginning with the narrative of the 'divided monarchy' the DtrH seems to make use of a new source: a 'Chronicle of the Kings of Judah' (Grabbe 2006b). Considering the widespread evidence for such chronicles in the ancient Near East, it seems unlikely that the author was inventing fictitious sources. The 'Chronicle of the Kings of Judah' seems to have provided the framework for the 'divided monarchy', but it was supplemented by much other material. The data that might have come from a chronicle are not extensive and could be explained as coming from a fairly concise chronicle such as we find in Mesopotamia.

The text refers to both the 'Chronicle of the Kings of Israel' and the 'Chronicle of the Kings of Judah', and it has usually been assumed that the author used two sources, which he synchronized himself. I have proposed that it is not necessary to assume two chronicles since all the chronicle data in 1 and 2 Kings could have come from a single chronicle, the 'Chronicle of the Kings of Judah'. The synchronic data about the kings of Israel would have been present in the 'Chronicle of the Kings of Judah'. This chronicle had several characteristics:

- Basic accession data but also including the name of the king's mother.
- Data on its main neighbour and rival, the kingdom of Israel, including the synchronic data.

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- Brief notes on major events in the king's reign. These were not necessarily stereotyped but could include a wide variety of data relating to the military, politics, building activities, personal matters relating to the king (e.g., major illnesses) and the cult. This was not according to a rigid formula.

As will be seen, some of the other sources may sometimes have contained reliable historical data, but most of the data in the text confirmed by external data as reliable could have come from such a chronicle. This means that the bulk of the DtrH's text is not of great value for historical events, though it can be of use for sociological study and of course for literary, theological and other non-historical disciplines of the Hebrew Bible. The use of stereotyped and formulaic language is not unusual in chronicles, but it is a way of organizing data, not necessarily of distorting it. So far, there is good evidence that the chronicle-type material in 1 and 2 Kings is fairly reliable (see further at §4.3).

4.2 Analysis

4.2.1 *Ahab*

The biblical text implies that the alliance with the Phoenicians was important to Omri and Ahab, with Ahab's marriage to Jezebel no doubt a means of sealing the alliance. This alliance is not directly attested in the non-biblical sources, but a number of the extra-biblical sources imply such a relationship. First, the name of Jezebel's father Ethbaal (*Ittobaal*) seems to have been mentioned in the sources used by Menander of Ephesus. Second, in the Kurkh Monolith Tyre and Sidon are not mentioned, either as paying tribute to Shalmaneser III or as members of the alliance of twelve kings. This would be explained if their armies were counted along with Israel's.

One of the areas often discussed is the number of chariots possessed by Ahab. The text clearly reads '2,000 chariots' (2 lim GIŠ.GIGIR.MEŠ), so the problems of trying to read a damaged text do not apply here. Yet the text is often emended. For example, D. Wiseman's translation in 1958 reads '200 chariots' without so much as a footnote or comment to indicate that the text has been emended (1958: 47). Perhaps the best defence of the view that '2,000' should be emended to '200' has been given by N. Na'aman, the main argument being that such a large force could not have been sustained by the Israelite economy (Na'aman 1976). However, this argument is not decisive for at least two reasons. The first is that the resources needed to

maintain a large force of horses is not the precise equivalent of the economic support needed for manufacturing and supplying a modern tank regiment. Two thousand chariots would need a large herd of horses, but these would not necessarily have been kept permanently in stalls. Grassland unsuitable for crops could still provide good grazing for horses kept in reserve until a national emergency arose. A second point to consider is that this force may not have been supplied by the kingdom of Israel alone (Kuan 1995: 39–47). Here the biblical text may be useful. 1 Kings 22.4 suggests that Judah was subordinate to Israel, perhaps being a vassal, as was Moab. Also as noted above, Tyre and Sidon are not mentioned in Shalmaneser's inscription, either as opposing his advance or as paying tribute. Since Tyre and Sidon are often listed as paying tribute in Assyrian inscriptions, it would be surprising if they were omitted by accident. However, if they were allies of Israel (as indeed they are so presented by the biblical text), they would be neither paying tribute to Assyria nor listed separately in the inscription.

Much has been made by some writers about the designation of Jehu as 'son of Omri', as some translations render it. One explanation is that the writer of the inscription did not know that Jehu was not a son of Omri but a usurper to the throne. However, the other inscriptions of Shalmaneser show that the Sumerogram DUMU 'son of' is used in a number of cases simply to designate a citizen of a particular country (hence, the translation 'man of'), though the person so designated usually happens to be the king. See the designation of Adramu king of Hamath as DUMU *A-gu-ú-si*, Aḫunu king of Adini as DUMU *A-di-ni*, and Ḫaiiānu king of Gabbari as DUMU *Gab-ba-ri* in the Kurkh Monolith (Grayson 1996: 17–18 [A.O.102.2: 15, 24, 27]) (also the discussion in Tadmor 1973: 149; Kuan 1995: 52–3 n. 167).

The question is, what happened between Ahab's coalition with Hadadezer and the oppression of Israel by Hazael? According to the biblical text Ahab fought the Aramaeans. This seems to go contrary to the Assyrian inscriptions and M. Astour (1971), for example, argued that Jehoram was recovering from wounds received fighting against the Assyrians rather than the Aramaeans when Jehu's coup took place. Astour interprets Jehu's revolt as a move by a pro-Assyrian faction. N. Na'aman (forthcoming) argues that Ahab was killed fighting the Assyrians. But some have been willing to argue that the biblical picture is not entirely wrong. That is, Ahab's alliance with the Aramaeans was a matter of necessity before a common enemy, but this did not prevent national concerns from taking over when the Assyrians were not threatening. Thus, the biblical representation of Ahab as fighting the Aramaeans toward the end of his reign is correct according to several interpreters (Bright [1980: 247] argued this). Basing himself on the Tell Dan inscription, Lipiński (2000: 373–80) has recently argued that not only had Ahab already fallen out with

Hadadezer but that Jehoram and Ahaziah were slain by Hazael, not Jehu. As already noted above, however, the reconstruction of the Tell Dan inscription is not as certain as this interpretation implies. Others are willing to believe that the Israelites and Aramaeans fell out late in the reign of Jehoram, and that the latter was indeed wounded fighting with Hazael over possession of Ramoth-Gilead (2 Kgs 9.14-15: Kuan 1995: 55-9).

4.2.2 *Israel and Moab*

According to 2 Kings (1.1; 3.4-5) Moab was under the dominion of Israel but managed to break free after Ahab's death. No suggestion is made as to who subjugated Moab in the first place. The Mesha Stela states that Omri 'oppressed' Moab for his lifetime and half the lifetime of his son, 40 years, before Mesha threw off the Israelite yoke. There is a remarkable coincidence between the biblical and the extra-biblical data, as was noticed when the Moabite stone was first discovered. However, there are some discrepancies that either cannot be reconciled or at least call for an explanation.

The first query concerns the name of Omri in the Mesha Stela. It has normally been taken as the name of a contemporary king of Israel. However, a new interpretation has recently been given by Thomas Thompson and Niels Peter Lemche. Lemche gives a cautious discussion, noting the absence of Ahab, the 'mythical' figure of 40 years, the reference to Jehu as the 'son' of Omri (in the Assyrian inscriptions), and suggesting that the 'mentioning of King Omri may therefore in this inscription not be solid evidence of the existence of a king of this name but simply a reference to the apical founder of the kingdom of Israel ... the dynastic name of the state of Israel' (Lemche 1998b: 44-6). If so, his 'son' could be any king of Israel down to the fall of Samaria. Thompson is more dogmatic:

Omri 'dwelling in Moab' is not a person doing anything in Transjordan. It is a character of story, an eponym, a personification of the state, *Bit Humri*'s political power and the presence of its army in eastern Palestine. We have a text. Therefore we are dealing with the literary, not the historical. From the historical name of *Bit Humri*, the Bible's story of a King Omri as builder of Samaria and founder of its dynasty grew just as much as had the story of King David and his forty kings. These sprang from the eponymic function of a truly historical *Byt dwd*. (Thompson 2000: 325)

Of course, the Mesha Stela gives a story, but so do *all* the inscriptions we are dealing with, including the Assyrian ones. To what extent a story is

historical has to be resolved on grounds other than genre. Is there evidence that Omri was a real person or only a personification of the state *Bit Humri*? That question cannot be answered directly because the two sources treating Omri as a person are the biblical text – which we are trying to test – and the Moabite stone – which is in question at the moment as a source. The only way to approach the question is to ask whether there are helpful analogies to this situation. There are in fact some good analogies in the person of Guš, the king of Yaḥan, an Aramaean state around Lake Gabbūl in northern Syria. We know he was an actual individual because he paid tribute to Ashurnasirpal II ca. 870 BCE (Grayson 1996: 218, lines 77–78 [text A.0.101.1]). Guš was apparently considered the founder of the state and dynasty. In 858 a ‘Hadram son of Guš’ paid tribute to Shalmaneser III (Grayson 1996: 17 [text A.0.102.2 ii 12]: *ḥa-ra-me DUMU gu-ú-si*; also p. 25 [A.0.102.3 96–97]). The reading of the name as ‘Hadrām’ (*ḥad-ra-me*) follows E. Lipiński (2000: 196 n. 12; 212) who has based this interpretation on other inscriptional material. The Zakkur inscription lists a ‘Bar-Gush’ among the kings arrayed against Zakkur (A 6). Other inscriptions refer to the state or dynasty as ‘house of Guš’ (*Bit-Gūsi/Bit-Agūsi* in Assyrian [Lipiński 2000: 196 and nn]; *byt gš* in Aramaic [KAI 223B: 10]). Similarly, Hazael is clearly attested as king of Damascus in a number of Assyrian inscriptions, but after his death the land of Damascus is sometimes referred to as *Bit Hazael* in Assyrian inscriptions (§4.1.5).

What these two examples illustrate is that a known historical individual (e.g., Guš, Hazael) can be taken as the founder or eponym of a dynasty, with the dynasty named after the person (‘house of X’) and the descendants even referred to as ‘son of X’. Thus, the expression *Bit-Humri* as a reference to the state/royal house of Israel does not rule out Omri as a historical personage. The question now is how to interpret the Moabite stone. Does the text suggest a eponymous ancestor when it mentions Omri or does it have in mind an actual king? First, the text refers to ‘Omri’ twice (lines 4–5, 7) but never to ‘house of Omri’. Second, there is no reference to ‘son of Omri’ as we have in some of the Assyrian inscriptions, where ‘DUMU/*mar* PN’ seem to be the equivalent of ‘king/man of (the house of) PN’. Instead, we have ‘his son’ (*bnh*: lines 6, 8), with a reference back to Omri alone (though *bn* probably means ‘grandson’ in this context; see next paragraph). This expression is not the same as that where an eponymous ancestor is used to designate an individual. The language of the Mesha Stela looks like a straightforward reference to an actual king of Israel, not just a mythical eponym of the Israelite royal house.

A second problem is the dating of Moab’s vassalage to Israel. A period of 40 years is mentioned, obviously a round number. Omri is associated with the

conquest of Moab; however, it is rather curious that Ahab is not mentioned at all, though the expression ‘his son’ might have been taken as a derogatory reference by not actually naming Ahab. We should not automatically assume that the Mesha inscription gives a correct account of the situation, since it clearly has its own biases; nevertheless, it is a near contemporary of the events, while the biblical account of Mesha is embedded in a prophetic legend and seems to have been written down or at least edited long after the events. This is not the whole story, however, because both accounts have to be treated critically. It is interesting that the length of the reigns of Omri and Ahab together total 34 years according to the biblical text (1 Kgs 16.23, 29), not far from 40 years. It may be that the reigns of Omri and Ahab have been telescoped – after all, the Moabite scribe was making a general point, not giving a blow-by-blow historical narrative – and ‘half the reign of his son’ could be a reference to Jehoram rather than Ahab (‘son’ being used generically for a more remote descendant, ‘grandson’ in this case). Moab rebelled in the reign of Jehoram according to 1 Kgs 1.1. Jehoram reigned 12 years, and if we add 6 of these to the 34 of Omri and Ahab, we have 40. This could just be coincidence, but it is not too improbable that the Moabite scribe was adding up the years of the reigns in similar fashion. If so, the two accounts in the Moabite stone and the Bible would in fact be quite close.

The reason for suggesting this interpretation is an obvious one: it seems unlikely that Moab could have rebelled under Ahab, considering the latter’s strength according to the Assyrian inscriptions. The logical time to have rebelled was after Ahab’s death, when a less experienced and weaker son was on the throne. Although the precise interpretation of the statement in the Mesha Stela is uncertain, it seems *prima facie* likely that the rebellion of Mesha took place after Ahab’s death – as the biblical text states – but it may also be that the Mesha Stela can be reconciled with this view, as just described.

4.2.3 *The Aramaeans*

The Aramaeans were very important for the history of the ancient Near East (Lipiński 2000; Dion 1997; Pitard 1987; O’Callaghan 1948). Their main impact was their language, which dominated the whole region from the Mediterranean Sea to Bactria from the Assyrian empire to the coming of Islam. They formed a number of small kingdoms, the kingdom of Damascus being especially prominent, but many of the others making little impact while we know little about them. The Aramaeans first appear for certain about 1100 BCE, in a tablet of Tiglath-pileser I (Lipiński 2000: 35–40). Some are

known to have settled in Mesopotamia, even in the cities, but others seem to have carried on a nomadic or semi-nomadic lifestyle in the region of Syria. They had the greatest impact on Israel during the ninth and eighth centuries BCE, especially the kingdom of Damascus. It is even suggested that Israel might have been a vassal to the king of Damascus, in the ninth century though this is not certain (Knauf).

One of the problems in the historical reconstruction of Aramaic history for this period is the tendency to multiply 'Ben-Hadad's. Some writers count up to four individuals, numbering Bar-Hadad I–IV (Dearman and Miller 1983). The reason for such a large number of individuals is that the biblical text has simply been harmonized with external sources, so that wherever 'Ben-Hadad' has been mentioned in the biblical text, it has been taken at face value (e.g., Bright 1980: 243; Dearman in Dearman/Miller 1983). This 'duplex' method of writing history is not convincing (on the question, see Grabbe 1997c: 64–5). This tendency has been resisted in more critical writers, and one recent book gives only two figures, a Bar-Hadad I and Bar-Hadad II (Miller in Dearman and Miller 1983 and Lipiński 2000: 407, who give only Bar-Hadad I and II as kings of Damascus). 1 Kings 21 refers to a 'Ben-Hadad' who was active in the time of Ahab, yet we know that the king of Damascus at this time was Hadadezer. An explanation of the biblical picture has become widely accepted in scholarship: 1 Kings 20–22 contains material from the later Jehu dynasty (e.g., 2 Kings 13) which has been mistakenly assigned to the reign of Ahab. The hypothesis has been championed by J. M. Miller, first in an unpublished PhD thesis, and then in a series of articles (Miller 1966; 1967; 1968; see also Pitard 1987: 114–25; Kuan 1995: 36–9). Part of the theory is that the original stories had only the titles 'king of Israel' and 'king of Judah', without the personal names, which made it easy to insert them wherever the editor thought fit. This hypothesis is cogent and fits the data well, which is why it has become so widely accepted in recent years. However, all it does is explain how the final text arose. The text as it presently stands is completely misleading, and the correct understanding (assuming one accepts the hypothesis) was possible only when extra-biblical sources became available.

The Assyrian inscriptions indicate that Ahab was an ally of Hadadezer. What, then, are we to make of the biblical passages that Ahab fought with 'Ben-Hadad' (1 Kgs 20–22)? The biblical writer is of course confused over the name of the king of Damascus during Ahab's rule and, as just suggested, the stories making Ahab fight the Aramaeans might have been transferred from the later history of the Jehu dynasty. But some have been willing to argue that while Ahab's alliance with the Aramaeans was a matter of necessity before a common enemy, this did not prevent hostilities from resuming when

the Assyrians were not threatening. Thus, the biblical representation of Ahab fighting the Aramaeans toward the end of his reign is correct according to several interpreters (e.g., Lipiński 2000: 373–80). This is of course possible, but it should be noted that Assyria was in a continual struggle with Hadadezer's coalition from his sixth year until at least the fourteenth year. Ahab is unlikely to have left the coalition during this time; it is only in the eighteenth year that a new king of Israel is mentioned, and the coalition under Hadadezer is defeated. If Ahab fought the Aramaeans, it would have been early in his reign, not in the last few years when he was part of the coalition organized by Hadadezer. It was only when Ahab was out of the way that the coalition was defeated.

Once the coalition assembled by Hadadezer had broken up, Hazael alone stood against the Assyrians. Campaigns against him are mentioned for Shalmaneser III's eighteenth (841 BCE) and twenty-first years (838 BCE). After that the Assyrians ceased to march into the western part of their empire for several decades, leaving Damascus to dominate the region. The picture of Hazael and his son Ben-Hadad (Bar-Hadad) causing trouble for Israel is a realistic one (2 Kgs 10.32–33; 12:18–19; 13.3–6). It is not attested directly because of the lack of Assyrian inscriptions, but the state of affairs is presupposed when the Assyrians once again intervened in the west under Adad-Nirari III. A coalition including Bar-Hadad, son of Hazael, attacked Zakkur of Hamath (Zakkur Inscription) and apparently went on to challenge the Assyrians. Not only does the biblical text have the sequence Hazael, followed by Bar-Hadad (Hebrew: Ben-Hadad), but the 'saviour' of 2 Kgs 13.5 is probably a reference to the Assyrian help. The Zakkur Inscription probably alludes to this same anti-Assyrian coalition, whereas Zakkur is pro-Assyrian and seems to have been delivered by Assyrian intervention. The Joash of Israel who is being relieved of the oppression from Bar-Hadad is mentioned in the el-Rimah Assyrian inscription (§4.1.5) as paying tribute to the Assyrians. Finally, the fall of Damascus (ca. 802 BCE: for the possible dates, see Kuan 1995: 93–106), as described in the el-Rimah and other inscriptions, would have taken the pressure off Israel and others who were under the yoke of Damascus. About 780–775 BCE Shamshi-ilu the Assyrian commander for the region collected tribute from Ḫadiiāni. This would suggest that Damascus was not able to do just anything it wished. Thus, although the biblical text cannot be confirmed in detail, the general picture given fits the situation in the last part of the ninth century as we know it from extra-biblical sources.

An important figure in the region and a significant opponent of the Assyrians was Rezin of Damascus (the Akkadian is variously ^m*Ra-bi-a-nu* kur^{kur} *Šá-imēri-šu-a-a* [Annal 13*, line 10 = Tadmor 1994: 68] and ^m*Ra-qi-a-*

nu ^{kur}Šá-imēri-šú-a+a [Stela IIIA, line 4 = Tadmor 1994: 106], apparently reflecting Aramaic *Radyan*). Although the ‘Syro-Ephraimite war’ (2 Kgs 15.29; 16.5-9; Isaiah 7) is not described as such in the Assyrian annals, it is compatible with everything so far known. In the end, though, both Rezin and his alleged ally Pekah lost out. Tiglath-pileser III took Damascus about 732 BCE (whether he killed Rezin is not preserved) and exiled many of the Aramaeans (Tadmor 1994: 138 [Summary Inscription 4, 7’-8’], 186 [Summary Inscription 9, reverse 3-4]).

4.2.4 Fall of Samaria

In what is an unusually detailed episode for comparative purposes, *Summary Inscription 4* tells about how Tiglath-pileser had Pekah removed for disloyalty and replaced by Hoshea (Tadmor 1994: 140 [Summary Inscription 4, 15’-19’], 188 [Summary Inscription 9, 9-11]). This is remarkably close to 2 Kgs 15.30. Perhaps the one discrepancy is whether Tiglath-pileser or Hoshea deposed Pekah, but this is probably a matter of wording. The removal of Pekah is not likely to have happened without Tiglath-pileser’s ultimate say-so, and one suspects that Hoshea acted only when he knew that he had Assyrian backing.

When we come to the siege and capture of Samaria and the deportation of many Israelites, there are some questions, even when we look at the Mesopotamian sources. In particular, what part did Shalmaneser V play and what hand did Sargon II have in the matter? Sargon II claims to have conquered Samaria, and some scholars have accepted these claims; however, the account in 2 Kgs 17.3-6 that this siege and capture of the city took place in the time of Shalmaneser V is supported by the *Babylonian Chronicles* (§4.1.5). The argument that the various sources can be reconciled is probably correct, though more than one solution is possible. B. Becking argued that there were two conquests of Samaria, one by Shalmaneser V in 723 BCE and another by Sargon II in 720 BCE in response to a rebellion of Ilu-bi’di of Hamath (1992; 2002). Following Becking (and Tadmor 1958b: 33-40), R. E. Tappy (2001: 558-75) argues similarly for two campaigns; however, M. C. Tetley (2002) has argued against the two-invasion hypothesis.

It seems likely that both Shalmaneser V and Sargon II were in some way both involved in the end of Samaria. Yet there is no evidence in the archaeology that the city was destroyed. Perhaps the biblical text does not clearly envisage a destruction of the city, but the archaeological evidence also seems to be against a wholesale deportation of the population (though a small portion does seem to have been removed and replaced by outside

settlers [§4.1.1]). As to the question of whether the Israelites were really taken to the places alleged in 2 Kgs 17.6 and of whether peoples from the places listed in 2 Kgs 17.24 were actually brought in, there is now some evidence that some movement of populations between the two regions actually took place (Becking 2001; 1992: 61–104; Na’aman and Zadok 2000; Oded 1979: 69–71; 2000: 91–9; Cogan and Tadmor 1988: 197, 209–10). But whether the extent of the deportation was as great as described in the biblical text is definitely to be queried.

4.2.5 Development of Religion

This section attempts to deal with the developments of Israelite and Judahite religion throughout the Iron Age. It is appropriate to do so in this chapter since the biblical text suggests a religious crisis under Ahab. The Bible pictures the Israelite religion as pure, monotheistic and different from the false worship of all other nations and peoples. This is the surface image, at least, and causes us little surprise. On the other hand, if one takes a broader view (including the evidence of archaeology), Israelite worship looks very much at home among the other Semitic religions, especially those of the Northwest Semitic region. This is not to deny that Israelite worship developed unique characteristics and came to establish strong boundaries against other religions, but this took a long time to develop (Albertz 1978; 1994; Smith 2001; 2002; Keel and Uehlinger 1998; Becking *et al.* 2001; Day 2000; Edelman [ed.] 1995b; Dietrich and Klopfenstein [eds] 1994; Nakhai 2001; Dever 2005; Gnuse 1997; DDD).

4.2.5.1 The God Yhwh

The deity most frequently and strongly associated with ancient Israel is Yhwh. The precise pronunciation is not known, though it is often reconstructed as *Yahweh* based on its appearance as *Iaō* (Ἰάω) in later Greek and Latin texts (e.g., Diodorus Siculus 1.94.2; Varro, apud Lydus, *De Mensibus* 4.53, pp. 110–11) and on the form of the name as it occurs in theophorous names in Masoretic vocalization in the biblical text (-*yāhû*). Apart from the biblical text, the name Yhwh is clearly attested first in the Moabite stone or Mesha Stela from the ninth century BCE (§4.1.4): Mesha took Nebo from Israel and dedicated the ‘vessels of Yhwh’ (כלי יהוה *kly Yhwh*) to his god Chemosh. Among the Khirbet Beit Lei inscriptions (about 600 BCE: Gogel 1998: #6.1.11; Davies 1991: #15.005) is a reference to a Yhwh who is

apparently the god of Jerusalem (the words ‘Yhwh god of’ [יהוה אלהי *Yhwh 'lhy*] and ‘Jerusalem’ are clearly visible). The name is also found in a seal from the early eighth century (Avigad and Sass 1997: #27), allegedly found in Jerusalem, which reads ‘Miqneyaw servant of Yhwh’ (מקניו עבד יהוה *Mqnyw 'bd Yhwh*). The ostraca from Arad (Aharoni 1981) dated to about 600 BC contain a number of blessings and invocations in the name of Yhwh (16.3; 18.2; 21.2, 4; 40.3). There is also a reference to the ‘house of Yhwh’ (בית יהוה *byt Yhwh*) which is probably the local temple (18.9). The Lachish ostraca (Gogel 1998: #6.1.15; TSSI #12), also evidently from the last days of the kingdom of Judah, contain a number of invocations using the name of Yhwh (‘may Yhwh give health/good news’; ‘as Yhwh lives’; see, e.g., Lachish 2.1-2, 5; 3.2-3, 9; 4.1; 5.1, 7; 6.1, 12).

The linguistic data attesting worship of Yhwh come from what were both the northern kingdom and the southern kingdom. Yhwh appears to have been a national or ethnic god, much as Chemosh was the god of the Moabites, Qaus the god of the Edomites, and so on (cf. 1 Kgs 11.33). This does not mean that Yhwh was the only god worshipped in these kingdoms, but he seems to have been the main object of devotion. His is the name most widely attested. Where did Yhwh originate? This has been a matter of intense debate over many decades, with various claims to have found the name in Ugarit, Mesopotamia and even Ebla. None of these has so far withstood scrutiny to general satisfaction. However, some Egyptian inscriptions of LB mention what may be a geographical name Yhwh, with reference to ‘the land of the Shasu Yahu’ (§2.2.1.4). Although the name *Yhw* seems to be geographical, it is possible that there is a connection with the divinity Yhwh, perhaps the region giving its name to the god worshipped there, or even possibly the deity giving the name to the region. However, arguments have been advanced from several quarters that Yhwh arose out of the context of El worship (see next section); this does not rule out a geographical origin (since the two theories could be combined), but it illustrates the difficulties.

The etymology of the name Yhwh has been much discussed. It would be impossible to survey and comment on the various suggestions made over the past two centuries, but two current theories need to be explored. Several biblical passages connect it with the verb *hyy/hwy* ‘to be’ (see below), and a number of scholars accept this and make the name a form of ‘to be’, but even if this etymology is accepted, there is still a controversy. Some take the name as a simple assertion (‘he is’/‘he exists’), but others (especially from the Albright school) have argued that there was once a common epithet of El as follows : ‘El who creates (heavenly) armies’ (‘*il dū yahwī šaba’ōt*). This verbal form developed into a name, the name of a separate deity. This

makes the verb a causative of 'to be' ('he causes to be' = 'he creates' [Cross 1973: 60–75]). The difficulty in knowing the exact vocalization and form of the name makes deciding between these theories a problem; even taking the name as 'Yahweh' does not solve the question since this is potentially a base form of the verb (*Qal*) or a causative (*Hiphil*). On the other hand, it has been argued that a derivation from 'to be' is excluded but that it comes from *hwy* 'to blow' (e.g., Knauf 1984), reflecting Yhwh's original function as a storm deity. If this is correct, the connection with 'to be' in biblical passages would be a folk (or even scribal) etymology. Deciding one's stance on the etymology of the name can have major implications for any theory about the origin of the Yhwh cult.

When we turn to the biblical text, the data may well represent different periods of time, and whether any of it can be projected back to the period before the Israelite monarchy is a major question. From the first chapter of Genesis the Bible abounds in the name Yhwh. It occurs frequently in the early chapters of Genesis, in the patriarchal narratives, and is the name by which the God of Israel reveals himself to Moses in Exodus. Yet a closer examination indicates some anomalies:

First, a great many personal names are theophorous, containing divine elements in them such as 'El 'God' (including such names as 'Ab 'Father', 'Ah 'Brother', and the like which are now thought to refer to the deity, perhaps conceived of as a divine father, brother, etc., or even a reference to a literal deceased relative now conceived of as deified). Yet no name is compounded with Yhwh before the narrative of Moses. The earliest name is Joshua (whose name was actually changed from Hoshea [Num. 13.16]). None of the Genesis genealogies contain names with Yhwh, and none of Moses' contemporaries are said to have such names. The name Yhwh appears in names in the biblical text only during the later life of Moses.

Second, the text of Exodus suggests that Yhwh was first revealed to Moses and not before his time. In Exod. 3.6 (usually assigned to the E source according to the Documentary Hypothesis), the God who appears to Moses identifies himself with the ancestral god of Abraham, Isaac and Jacob. The text goes on to say that this God is called 'I am' (אֲנִי אֶהְיֶה 'ehyeh), and immediately connects this name with 'Yhwh the God of your fathers, the God of Abraham, the God of Isaac, and the God of Jacob' (Exod. 3.13–16). Exodus 6.3 (usually assigned to the P source) states that God appeared to Abraham, Isaac and Jacob under the name of El Shaddai but was not known to them by his name Yhwh. Thus, if these passages have any weight, they suggest that the name of Yhwh was not known in Israel before the time of Moses. It was these passages, among others, that A. Alt used to develop his theory of the 'god of the fathers' (1966). Yhwh is indeed the 'god of the

fathers' in these passages, but most of Alt's thesis does not stand in the light of more recent discoveries (Cross 1973: 3–43).

This is only a simple analysis – even a simplistic one – of the biblical text and ignores the whole question of Moses' existence and the relationship of the biblical traditions to the actual settlement of the Israelite people in the land. However, we can still note two points: it does conform with extra-biblical data and it suggests memory of a time when Yhwh was not known to the Israelites but was introduced to them for the first time. For a writer to dream up a period when Yhwh was not known would be very surprising indeed, if everyone assumed that Yhwh had been in Israelite possession from the beginning. Thus, to the best of our current knowledge Yhwh originated in Palestine and his worship was confined to the peoples of Palestine. There is considerable – if not universal – support for the view that the early Egyptian inscriptions mentioning *Yhw* do in fact refer to the name Yhwh and that worship of Yhwh did not originate with Israel but was picked up sometime during the pre-settlement or settlement period (Blenkinsopp forthcoming).

However, evidence has more recently been found for Yhwh worship in the area of Syria in a text from Hamath (Dalley 1990). Based on this text, it has even been suggested that Yhwh was not unique to Israel/Judah but rather a deity common to a large part of Syro-Palestine (Thompson 1995a: 119 n. 13). The reference seems to be certain, but there is no supporting evidence that Yhwh was part of general worship over the region. Rather, it looks more likely that Yhwh worship had been transplanted from Palestine in some way, perhaps by a royal family which had also moved from the Palestinian region or some other means, but remained isolated or confined to a minority. In all the inscriptions and linguistic data from the surrounding region, there is nothing to indicate that Yhwh was worshipped generally over the entire region.

4.2.5.2 Other Deities and Worship

The references to the God of Israel in the biblical text do not just contain the name Yhwh. Many other names and titles occur, though often translated in Bible translations as epithets or descriptions rather than rendered as names. For example, the name *'El* is used of the Israelite God but can also mean just 'god' in a generic sense. This is in line with much Northwest Semitic usage in which *'el* (or the earlier form *'ilu*) could stand both for the head of the pantheon (the god El) and for the word 'god, divinity' in general). The name El Shaddai (or just Shaddai alone) is also used in a number of passages in Genesis (17.1; 28.3; 35.11; 48.3; 49.25). It is also the divine name found

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through most of the poem of Job and seems to be the prime divinity for the original composer of the book's core (the name Yhwh occurs in Job only in the framework). Genesis 14.18 attests the deity El Elyon, not known anywhere else. (Some scholars doubt the accuracy of the text here, though the name Elyon by itself also occurs in Num. 24.16, Deut. 32.8 and Isa. 14.14, while the Ugaritic equivalent [*ly*] is applied to Baal [KTU 1.16.3.6, 8].)

Other biblical texts also suggest a time when Yhwh was not only a deity alongside other deities but perhaps even a subordinate of El. Scholars had long wondered whether the reading of the Hebrew text of Deut. 32.8 was not due to a later editing because of the Septuagint text which seemed to presuppose a different Hebrew original: the god Elyon 'established the boundaries of the nations according to the number of the angels of God'. The suggestion of another more original Hebrew reading seems now to have been confirmed by a Hebrew manuscript from Qumran (4QDeutⁱ = 4Q37) which reads '[according to the number of] the sons of God' (בְּנֵי אֱלֹהִים *bny 'lhyim*). The passage goes on to say that Jacob is Yhwh's portion (32.9). All of these data suggest that the passage originally read something along the lines of the following:

When Elyon gave the inheritance of the nations,
When he divided the sons of Adam (or man),
He established the boundaries of the peoples
According to the number of the sons of El.
For Yhwh's lot is his people
And Jacob his inherited portion.

This suggests that Yhwh (as one of these sons of El) inherited Israel as his particular portion.

Such a situation in which Yhwh is merely one among the sons of El in the divine assembly is found in Ps. 89.7-8 which reads literally:

For who in heaven compares to Yhwh?
Who is like Yhwh among the sons of the Elim (gods)?
El creates awe in the council of the Holy Ones.
He is great and strikes fear in all about him.

Here Yhwh is a son of El, among other sons, even if he is said to be incomparable to his fellow sons of El. Similarly, Ps. 82.1 speaks of God judging among the gods.

The concept of the divine assembly or divine council is one widespread among Semitic pantheons. Perhaps one of the clearest examples of Yhwh

himself having a divine council is 1 Kgs 22.19-22 in which he presides over the 'host of heaven' (צְבָא הַשָּׁמַיִם *ṣēbā' haššāmayim*). In later times this divine council was interpreted as consisting of angels who surrounded Yhwh, but angels have only a minor place in Israelite tradition until the post-exilic period. The Hebrew word for 'angel' is מַלְאָךְ *mal'ak*, meaning 'messenger' and may be used of human messengers as well as heavenly ones. Studies of the Northwest Semitic pantheons have shown that the gods had various ranks, the lowest being the messenger gods (Handy 1994). These messengers spoke for the gods sending them. Similarly, angels in the Bible speak for Yhwh; for example, Exod. 3.2-5 mentions that 'the *mal'ak Yhwh*' appeared to Moses, but then the rest of the passage goes on to say that 'Yhwh said'. Thus, the messenger gods of the original Northwest Semitic pantheon (and perhaps other divinities as well) became reduced to angels in later Judaism, but the outlines of the original polytheistic divine council are still retained despite the monotheistic views of the final editors of the tradition.

A number of other passages also give a mythical picture of Yhwh not found in most biblical texts (Day 1985). They remind one of myths about Baal known from the Ugaritic texts and even have overtones of Marduk's defeat of Tiamat from whose body he created the heavens and the earth in the Babylonian creation epic *Enuma Elish*. Unlike Genesis 1, these passages suggest that God created by defeating various monsters of chaos who appear as supernatural beings. For example, in Isa. 27.1 Yhwh takes his sword and defeats 'Leviathan (לְוִיָּתָן *lwytn*) the piercing (בֶּרֶךְ *brh*) serpent, even Leviathan the slippery (עֲקָלְטוֹן *'qltn*) serpent, and he will slay the Tannin (תַּנִּין *tnyn*) in the sea'. This can be compared with Ugaritic texts which allege that Baal defeated such monsters. One prominent Baal myth describes Baal's defeat of the sea god Yamm (*KTU* 2). Another passage alludes to battles which so far have not come to light in detailed texts. *KTU* 1.3.3.37-42 tells of other monsters of chaos defeated by Baal:

Did I not destroy Yamm the darling of El,
 did I not make an end of Nahar the great god?
 Was not the dragon (*tmn*) captured and vanquished?
 I did destroy the wriggling (*'qltn*) serpent,
 the tyrant with seven heads.

Similarly, *KTU* 1.5.1.1-3 states:

For all that you smote Leviathan the slippery serpent (*ltn.bṭn.brh*)
 and made an end of the wriggling (*'qltn*) serpent ...,
 the tyrant with seven heads?

Cf. also *KTU* 1.5.1.27–30. The Hebrew and Ugaritic texts are not only similar in theme but even share some of the same basic vocabulary.

These various passages are isolated survivals of older beliefs which had been obliterated or reinterpreted by the dominant monotheistic view of Yhwh that controlled the final shaping of the biblical text. A few verses escaped editing, however, confirming what we now know from inscriptions: Yhwh was originally conceived as one god among many, perhaps even subordinate to and a son of El. He created by fighting and defeating various monsters of chaos, such as Leviathan, Tannin and Rahab, much as Baal did in the Ugaritic texts. When monotheism became the dominant view, these older views were simply expunged or, in some cases, they were reinterpreted so as not to be an embarrassment to monotheistic views.

Biblical names also suggest a plethora of deities. The names with El are too numerous to mention (Israel, Elijah, Elisha, Samuel and so on); more significant are Shaddai (e.g., Num. 1.5-6: Shedeur, Zurishaddai; Num. 1.12: Ammishaddai). But perhaps most interesting are the names with Baal. Considering the biblical polemic against Baal, one might have expected not to see such names, but they are found in surprising contexts. One of Saul's sons has a name compounded with Baal: Eshbaal ('man of Baal') and Jonathan's son was Meribbaal. These names are often overlooked because the Samuel texts actually substitute surrogate names compounded with the word 'shame' (Ishbosheth [2 Sam. 2.8]; Mephibosheth [2 Sam. 21.7]), but they are correctly preserved in 1 Chronicles (8.33-34; 9.39-40).

Should we be surprised at all this since, after all, does not the biblical text show that Israel had a penchant for 'falling away' from true worship into paganism, idolatry and worship of other gods? To balance this, one might well point to the opposition to Baal worship in the time of Elijah. However, it is not entirely clear that this is as simple as it looks, for the 'Baal' of Jezebel was most likely a Phoenician god – and thus a foreign cult – introduced into Israel. It was symbolic of a foreign queen and would have been opposed by certain traditionalists. The fact that all the Baal worshippers could supposedly fit into the small Baal temple (2 Kgs 10.18-28) is evidence that this was not a widespread alternative to Yhwh worship. Further indication is found in the names of Ahab's family and associates. His chief minister was named Obadiah ('servant of Yhwh': 1 Kgs 18.3) and his two sons had Yhwh names (Ahaziah and Jehoram); the prophets he consulted were prophets of Yhwh (1 Kgs 22.5-28). Although the text accuses him of Baal worship (1 Kgs 16.31-32), we see no actual evidence that he promoted Baal worship beyond the royal cult specifically established for his wife. The opposition of Elijah and others was probably political opposition to Jezebel, even if disguised as religious piety. Ahab himself, by all accounts, was a Yhwh worshipper.

The ostraca found during the excavation of Samaria give a similarly mixed picture (§4.1.2). A variety of theophorous names are found in the texts known so far; of these eleven are compounded with Yhwh while six have Baal. There is no way to see any social distinction in the names: one has the impression that these were common, ordinary names about which people would not have thought very much except perhaps in a cultic context. It would appear that Baal and Yhwh were worshipped happily side by side.

Recent finds have been even more revealing. An inscription found in 1975–76 at Kuntillet Ajrud in the Negev (dated about the eighth century BCE) is conventionally read, ‘I blessed you by Yhwh of Samaria and by his Asherah’ (Gogel 1998: #6.1.14; Davies 1991: #8.017, #8.021). Similarly, in Khirbet el-Qom near Hebron, another inscription was been found and dated to the seventh century (Dever 1970; Gogel 1998: #6.1.12; Davies 1991: #25.003). It is very difficult to read – partly, because it seems to have some of the letters duplicated – and a number of readings have been given (Zevit 2001: 359–70): the original editor A. Lemaire translated it as: ‘Blessed be Uriah by Yhwh and <by his Asherah>, from his enemies he has saved him’. The epigrapher Joseph Naveh read it: ‘May Uriyahu be blessed by Yahweh my guardian and by his Asherah’. Ziony Zevit interpreted it as: ‘I blessed Uryahu to YHWH / to wit, from his enemies ... for the sake of Asheratah save him / by Abiyahu’.

These finds have created a great deal of debate because this is the first time that any direct evidence of goddess worship had turned up (even the Samaria ostraca had no goddess names). There was naturally some question as to whether the ‘Asherah’ was a reference to a cult object or to a goddess. It partly hinges on the grammatical question of whether one can put a pronominal suffix on a proper name in the Northwest Semitic languages. Some have suggested alternative interpretations (e.g., that the final letter - *h* is not ‘his’ but a so-called locative *hē* or perhaps even a vowel letter), but others argue that the grammatical problems with a pronominal suffix can be overcome (Zevit 2001: 363–6). After considerable disagreement, the consensus is moving definitely in the direction of seeing a consort with Yhwh, a female divinity called Asherah (even if not everyone agrees, e.g., Keel and Uehlinger 1998: 281–2). If so, this would be quite parallel to Ugarit in which El the head of the pantheon has Athirat (cognate with Hebrew Asherah) as his consort.

The biblical text itself suggests goddess worship in several passages: it refers to ‘Asherah’, though at times this seems to designate a cult object, especially when appearing in the masculine plural (‘Asherim’). Yet a number of passages seem definitely to refer to a goddess. 1 Kings 15.13 mentions the cult object made for Asherah. This was presumably in the temple; indeed, 2

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Kgs 23.4, 7 mentions vessels of Asherah (among others) and cult personnel dedicated to Asherah in the Jerusalem temple, and 2 Kgs 21.7 also speaks of an image of Asherah in the temple. 1 Kings 18.19 designates 'the prophets of Asherah', alongside the prophets of Baal, which can only be a reference to a goddess. Thus, the biblical text itself preserves evidence that Asherah was worshipped – even in the Jerusalem temple – most likely as a consort of Yhwh. Worship of the 'host of heaven', referred to in 2 Kgs 17.16; 21.3; 23.4-5, is confirmed by solar symbols found on a number of Israelite seals (Keel and Uehlinger 1998: 282–309). Jeremiah (44.17-19, 25) mentions worship of the 'Queen of Heaven' who is likely to have been Asherah or Anat or perhaps even an amalgam of the two goddesses. Yet even though the text presents these as acts of apostasy, there is no hint that such worship was criticized or opposed at the time. If there was criticism, it was likely to have been a minority movement, perhaps a 'Yhwh-alone movement' (as argued by Morton Smith 1971).

One other indication is an inscription of Sargon II relating to the fall of Samaria about 722 BCE. The Nimrud Prism states as follow (4.29-33, minor restorations not indicated):

- 29) With the power of the great gods, my lords,
- 30) against them I fought.
- 31) 27,280 persons with their chariots
- 32) and the gods in whom they trusted, as spoil
- 33) I counted.

It was quite normal for the Assyrians to remove the divine images of the people they conquered, often melting them down for the metal. The most reasonable interpretation in the context is that these referred to images of the Samarian gods. That is, the temple(s) of the Samaritans contained images of more than one god, and the Assyrians took these away as spoil as was their custom.

Finally, the Jewish military colony at Elephantine was almost certainly pre-Persian, probably being established during the Neo-Babylonian or possibly even in the Assyrian period before the fall of Jerusalem in 587/586 BCE. The community had its own temple to *Yhw* (probably pronounced Yahu or Yahoo) until it was destroyed by some of the local Egyptians (*TAD* A4.7–9 = *AP* 30–32); whether it was ever rebuilt, we do not know. However, a list of contributors to the cult indicates that other divinities also had a place. Specifically listed are Eshem-Bethel and Anat-Bethel (*TAD* C3.15:127–8 = *AP* #22:124–5) and Anat-Yahu (*TAD* B7.3:3 = *AP* #44:3). It has been suggested that these were actually only hypostases of Yhwh (cf. Porten 1968: 179). This

is a difficult matter to resolve, partly because these names occur only here – as if they had ceased to have much of a function in the Elephantine religion. They no doubt originated as goddess figures, but they may have developed in other ways (see further Grabbe 2004a: 241–2). Perhaps we are witnessing an evolution toward monotheism in this late period (see further below).

Archaeology has also given a good deal of information which confirms this textual picture (Nakhai 2001; Dever 2005: 110–75; Vriezen 2001). Only some brief references can be given here, though some have been discussed elsewhere (e.g., the Arad altar [§5.1.1]). Just to give a couple of examples, one of the most striking objects is the Taanach altar, usually dated to the tenth century and found at what seems to be a *bamah* or open air cult site (Lapp 1969: 42–4). The goddess Asherah seems to have a prominent place on it. Another striking object is the bronze bull of the ‘Bull Site’ (Mazar 1982). Again, it was found as the focal point of a cult site. It is not always possible to tell who used a particular cult site or the god represented (is the bull Yhwh or Baal?), but the archaeology amply demonstrates the variety of worship that texts have delineated.

4.2.5.3 Temple Religion versus ‘Popular’/‘Folk’/‘Family’ Religion

There are some problems with using the term ‘popular’ or ‘folk’ or ‘family’ religion. First, this might imply that such religion is less important or even less religion. Such is not the case: popular/folk/family religion is as legitimate and as much religion as any other. A second problem is a tendency to see this type of religion in a different category from other types of religion. This is erroneous: while perhaps having its own social and practical characteristics, it does not differ from any other type of religion. In the ancient Near East we can perhaps talk about three spheres of religion: the cult of the ruler, the national cult or cult of the national god (which might be the same as the ruler cult but not necessarily), and local or family or ‘popular’ cults. In Israel and Judah the distance between the temple cults and family cults might not have been that great. Yhwh worship seems to have been widespread, and with a multitude of altars it was common for ordinary people to attend worship in a temple (cf. 1 Samuel 1). What tends to characterize popular religion is the devotion to a particular deity for personal concerns and favours and the practice of the ‘esoteric arts’ for personal benefit: cults (including underworld cults) relating to the dead, healing, curses/blessings, magic, divination (Grabbe 1995: 119–51).

Temple religion came to be perhaps the main manifestation of Israelite religion before it became a book religion, but both these were probably

post-exilic developments. That is, temples existed in pre-exilic times, including in Jerusalem, but it was probably only in the Persian period that the Jerusalem temple became the only one in many people's eyes (though there still existed temples on Gerizim, at Elephantine, in Leontopolis and possibly at Iraq al-Amir in Transjordan). As for Judaism as a 'religion of the book', I have argued that it was a development only toward the end of the Second Temple period (Grabbe 2000a: 317–18). The centre of temple religion was the sacrificial cult (Grabbe 1993a: 29–43; Nakhai 2001). Although the book of Leviticus is a late writing, it is likely that the cult described there had continued in broad outline for many centuries (Grabbe 1993a). The book of Leviticus is not a handbook for priests as some have suggested (Grabbe 2004b). The priestly knowledge was most likely passed down by word of mouth and apprenticeship training without being committed formally to writing (at least, until very late). But we seem to have some knowledge of the cult and priesthood in the Persian period (Grabbe 2004a: 216–37; Schaper 2000) which can, with suitable caution, be projected back into the period of the monarchy. One of the differences was that during the monarchy the king was the chief religious figure – the chief priest, you could even say (Grabbe 1995: 20–40).

A cult of the dead was evidently a widespread phenomenon in ancient Israel, attested both from the text and archaeology (Bloch-Smith 1992; Grabbe 1995: 141–5) – and, incidentally, another indication of polytheism. A cult of the dead can take many forms and have many connotations, but one type is to treat the deceased ancestors as joining the Rephaim and becoming deified. Although the biblical text treats the Rephaim as early inhabitants of Canaan in some passages, texts from Ugarit and Phoenicia and even the Bible (Job 26.5; Ps. 88.11–13; Isa. 26.14, 19; Prov. 9.18) indicate the Rephaim are associated with the dead. Necromancy was evidently quite widely practised in ancient Israel (cf. Deut. 18.9–14; 1 Sam. 15.22–23; 28; Ezek. 21.26–27). The text in its present form naturally condemns such practices, but it equally attests its popularity. Another cult associated with the dead and the underworld is Molek worship. Whether Molek is a deity or only a sacrifice is still debated (cf. Heider 1985; Day 1989; Smelik 1995), but in any case, it seems to have been a chthonic cult associated with the underworld and with a cult of the dead. How widespread this cult was in Palestine is debatable, but it certainly existed, even in Jerusalem. It may also have involved child sacrifice, a subject still controversial to debate (2 Kgs 16.3; 17.17, 31; 21.6; Jer. 7.31–32; Day 1989; Smelik 1995). Finally, there are the teraphim mentioned in a number of passages (1 Sam. 15.22–23; 2 Kgs 23.24; Ezek. 21.26–27; Hos. 3.4; Zech. 10.2). Recent study of the teraphim argues that they were a common shrine to the ancestors kept in the home (Toorn 1990). Even the future king David

had one, which his wife placed in his bed to deceive Saul's men so that he could escape (1 Samuel 19).

A subject of considerable controversy is that of cultic prostitution. According to some of the Greek writers, worship in Babylonian temples required all women to prostitute themselves at one time in their lives (Herodotus 1.199). We also have some evidence of 'sacred marriage' rites in Mesopotamia, which might have involved the king and queen or the king and a priestess in ritual sex (Kramer 1969), though this is now disputed (Lapinkivi 2004). Among the cult personnel at Ugarit are those called *qdšm*, which some have proposed were male cultic prostitutes. This idea is now best rejected; there is no evidence for ritual prostitution – male or female – at Ugarit (Tarragon 1980: 138–41; Soden 1970). The biblical text (and subsequent Jewish and Christian religious traditions) asserts decadent sexual practices in 'pagan' worship, especially among the so-called Canaanites. It was therefore easy to believe in 'abominations' such as sexual orgies, prostitution, sodomy and the like as a constituent part of the cults which Israel was to eradicate (cf. Yamauchi 1973). As already suggested above, recent study has now called into question many of the assumptions about the 'Canaanite' cults and other worship in Israel's environment, as well as the biblical version of the development of Israelite religion. Most of the biblical references can be interpreted as symbolic language for religious straying, and there is really little support for the oft-repeated assertion that cultic prostitution formed a part of the non-Yahwistic cults (Barstad 1984: 21–33; Goodfriend and Toorn 1992).

4.2.5.4 Development of Monotheism

How early monotheism developed is very much a point of dispute, though the general view today is that it was fairly late. The final form of the text in most books bears the stamp of those who were apparently monotheistic. In the biblical text Yhwh is clearly equated with El and with other divine names. Since Asherah is most often a consort of El in Northwest Semitic texts, a number of recent scholars have suggested that this equation is not fortuitous but represents the actual origin of Yhwh, that is, that the latter developed in the context of El worship (§4.2.5.1). In any case, it would not be surprising for El and Yhwh to be assimilated over time even if they were once separate deities. The various other male deities are equated with Yhwh (except for Baal who is depicted as being in a life-or-death struggle with Yhwh). This is a part of the process of moving toward monotheism. Many would argue that we find monotheism already in Second Isaiah with his denial of the existence

of other gods (e.g., Albertz 1994: 417–18; Smith 2001: 179–94; 2002: 191–9; Becking *et al.* 2001: 191–2; contrast Thompson [1995a: 113] who states that Second Isaiah is an example of ‘inclusive monotheism’): Yhwh alone is without beginning or end, uniquely divine and God alone, the creator of the cosmos and there is nothing like him (Isa. 46.9; 48.12–13).

The book of Deuteronomy has some statements that express similar ideas (e.g., Deut. 4.35, 39), but these exist alongside others that seem to accept the existence of other gods (Deut. 5.7; 6:4 [cf. Smith 2001: 153]). This could be explained by an editing process in which the monotheistic statements were later additions to the text. Thus, many scholars see the development of monotheism – in some circles, anyway – during the seventh to sixth centuries BCE. For example, it has been pointed out that astral imagery of the late eighth and seventh centuries had disappeared from seals and seal impressions of the Jerusalem elite by the early sixth and also the blessing and salvation functions of Yhwh’s ‘Asherah’, known from several inscriptions, had been absorbed by Yhwh by the time of the Lachish and Arad ostraca (Uehlinger 1995; 2005). This has been interpreted as indirect evidence of Josiah’s reform and a move in the direction of monotheism.

A topic closely associated with the issue of monotheism is the question of whether the worship of the Israelite god was aniconic. A number of biblical texts forbid the use of images in worship, such as the second commandment (Exodus 20.4; Deut. 5.8) and images are ridiculed (Isa. 40.18–20; 44.9–20; 46.1–2; Jer. 10.2–10). We know from later Jewish texts that the use of images or living forms of any kind was considered abhorrent and strongly rejected (Wisdom 13–15; *Liber Antiquitatum Biblicarum* 44; *Testament of Job* 2–5; *Apocalypse of Abraham* 1–8; see also the quote from Hecataeus below). Some have argued that the worship of Yhwh was aniconic from the beginning; that is, that the non-use of images was a characteristic of Yhwh worship in the earlier period. Yet there are a number of indications that images were used in divine worship in Israel and Judah. The quotation from Sargon II above (§4.2.5.2) is already one indication. The many ‘Astarte’ images found by archaeologists all over Palestine is another (Moorey 2001; Stern 2001: 205–11). These by themselves do not prove that idols were used to represent Yhwh since the passages just mentioned could be referring to other deities, but the possibility that an image of Yhwh was to be found in the Jerusalem temple at one stage is a reasonable inference from the data just presented.

Apart from the biblical text, whose interpretation some dispute, one of the first and best indications of monotheistic, aniconic worship is found in Hecataeus of Abdera, a Greek writing about 300 BC. He describes the Jews in Palestine in a long paragraph which was then picked up and quoted by Diodorus of Sicily (40.3.4). The significant passage states:

But he [Moses] had no images whatsoever of the gods made for them, being of the opinion that God is not in human form; rather the Heaven that surrounds the earth is alone divine, and rules the universe.

4.3 Synthesis

By the ninth century we seem to have reached a period where there is more agreement about the general outline of history in Palestine. There seems to be general agreement that Omri and Ahab ruled over some sort of state, even if the exact nature of that state is still debated (H. M. Niemann [2007], for example, refers to Omri as the war leader of a military administration). Judah was also a separate entity, but whether it is called a state is more controversial: it might have been a vassal of Israel, which in turn was a vassal of Damascus part of the time, though not under Omri (Knauf 2007). Jerusalem could well have been much what it was in the Amarna period, perhaps more of a city-state than anything else. Most seem to agree, though, that sometime in the eighth century Judah had become a territorial state.

In the ninth century the brief hiatus of freedom from the major powers, which Palestine had enjoyed since Egypt withdrew, came to an end. Shoshenq seems to have conducted some sort of excursion(s) into Palestine, though the extent and the effects are currently moot, but in any case he did not stay. But then in the early ninth century Assyria began to make excursions to the west, and in the middle of the century are the repeated clashes between Shalmaneser III and a coalition of twelve kings from the Syro-Palestinian region, led by Damascus but with Ahab an important ally. According to the Assyrian inscriptions the coalition remained intact up to Shalmaneser's fourteenth year (845 BCE). Between then and the eighteenth year (841 BCE) things must have fallen apart, for in 841 the coalition was defeated, and with Hadadezer now dead Damascus, under a new king Hazael, alone faced the Assyrian army. In the same year the new king of Israel, Jehu, submitted to Assyria, putting it on the opposite side from the Aramaeans. The Assyrians continued to cause problems for Hazael for another three years, until Shalmaneser's twenty-first year, but then Assyria ceased to concern itself with the western part of its empire for the next 30 years. This left Damascus a free hand to dominate the region, including Israel, first under the leadership of Hazael and then under his son Bar-Hadad. This is exactly the picture of the biblical text (2 Kgs 10.34-36; 12.18-19; 13.3, 22-25).

After the events around 800 BCE, there is not a lot of further extra-biblical information for the next half century until the reign of Tiglath-pileser III. The biblical text assigns a good deal of important activity to Jeroboam II,

but this individual is not mentioned by any extra-biblical sources. About 780–775 BCE Shamshi-ilu the Assyrian commander for the region collected tribute from Ḥadiiāni. This would suggest that Damascus was not able to do just anything it wished. The next king of Israel mentioned is Menahem, in Tiglath-pileser’s *Annals* and in the Iran Stele III (ca. 738). For the first time in the Assyrian annals, we also find a mention of the kingdom of Judah, in Tiglath-pileser’s *Summary Inscription* 7. According to *Summary Inscription* 4, the Assyrian king also annexed Gilead, Galilee and other areas of northern Israel (cf. 2 Kgs 15.29).

The Syro-Ephraimite war in 734–732 BCE is not mentioned as such in the Assyrian records, but it is indirectly attested. The exact cause of Damascus and Israel ganging up on Judah still escapes us, but a number of biblical passages presuppose it. It also brought Judah into Assyria’s sphere, though this was likely to have happened shortly, anyway. It presaged the final fall of the northern kingdom. Here again the original sources give more than one picture. Was Samaria captured by Shalmaneser V or Sargon II? The proposal that it was taken twice – in 722 and again in 720 BCE – has merit, but there are other explanations, such as Sargon claiming to capture the city when all he did was deport its inhabitants. But the basic picture of the biblical text seems to be in harmony with the Assyrian sources, even if the details are not always reliable.

We can now summarize the results of this chapter as far as history is concerned. In doing so, it is difficult to indicate graphically the relative importance of the points listed below.

4.3.1 Biblical Data Confirmed

- A number of the Israelite kings and their approximate dates are confirmed: Omri, Ahab, Jehu, Joash, Menahem, Pekah, Hoshea, as well as probably Jehoram. Of the kings of Judah, Jehoahaz and possibly Ahaziah are correctly remembered.
- Mesha king of Moab is correctly remembered and his breaking away from Israelite rule (though some of the details remain questionable).
- Of the Aramaean rulers, Hazael of Damascus, his son Ben-Hadad, and Rezin are given correctly.
- Strength of Hazael over Israel and the region.
- Strength of Ben-Hadad, son of Hazael, and his loss of power over Israel (the Assyrian intervention is not mentioned explicitly, though it may be alluded to).
- Menahem’s payment of tribute.

- Pekah's defeat and death.
- Assyrian defeat of Rezin of Damascus.
- Hoshea of Israel as replacement for Pekah.
- Jehoahaz (also called just Ahaz) of Judah's interaction with Tiglath-pileser III.
- Fall of Samaria to Shalmaneser V.

4.3.2 Biblical Data not Confirmed, Though They may be Correct

- Ahab may have fought the Aramaeans during part of his reign, though not at the end.
- Whether any of the stories about Elijah and Elisha preserve data about the doings of actual individuals is hard to say. It would not be surprising if such individuals actually lived, but it would be very unlikely that the details of their lives could be verified. Of course, most of us would discount miraculous happenings and prescient knowledge.
- Jehu's revolt and coup.
- Rule of Athaliah.
- Reign of Jeroboam II.
- Syro-Ephraimite War.
- The other kings of Judah (in addition to Ahaz and perhaps Ahaziah).
- 'Minor kings' of Israel, such as Zechariah and Pekahiah.

4.3.3 Biblical Picture Incorrect

- Ahab is presented as weak militarily.
- The Aramaeans are the main enemy.
- The king of Aram is wrongly given as Ben-Hadad in the time of Ahab.

4.3.4 Biblical Picture Omits/has Gaps

- The Assyrians are strangely absent from the picture until the time of Tiglath-pileser. It is possible that the biblical writer had no knowledge of the Assyrians in the time of Ahab, but if he did, he has suppressed this information. If material from the later Jehu dynasty is found in 1 Kings 20 and 22, as many scholars now believe, the biblical writer may have placed it in the present position by mistake. The alternative – that the editor/compiler knew the true nature of the material but

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deliberately misused it – is by no means impossible, but the ignorant use of the material is more likely.

- Jehu's submission to Shalmaneser III, along with his payment of tribute, is not mentioned in the Bible.
- The submission of Joash of Israel to the Assyrians.

CHAPTER 5

Iron IIC (720–539 BCE): Peak and Decline of Judah

The end of the eighth century marked a major watershed in the history of Israel. The Kingdom of Israel came to an end by about 720 BCE, leaving only Judah as a semi-independent kingdom. This might seem to have removed a major rival for Judah, but the kingdom was now an Assyrian vassal, and Assyria was now in close proximity in the form of the Assyrian provinces to Judah's north: Samerina, Megiddo and Dor. Then came Sennacherib's invasion in 701 BCE, which was by any reckoning a crucial event in Judah's history. In many ways, the period of Judah's history between Sennacherib's and Nebuchanezzar's invasions is the subject of this chapter.

5.1 Sources

5.1.1 Archaeology

This period in Judah's history began with the invasion of Sennacherib, which left Judah devastated (see the survey in Grabbe [ed.] 2003: 3–20). Only Jerusalem and a few other (mostly northern) sites (e.g., Mizpah [Tell en-Nasbeh] and Gezer) escaped the Assyrian wrath. Major sites destroyed included Ramat-Rahel, Timnah (Tell Batash), Beth-Shemesh, Tell Judeidah, Tel 'Erani, Tell Beit-Mirsim, Tell Halif (Tell Khuweilifeh), Khirbet Rabud (Debir/Kiriath-Sepher) and Arad. Beersheba is often thought to have been destroyed by Sennacherib, but E. A. Knauf now argues against this (2002). Of 354 Judaeon settlements in existence in the late eighth century and destroyed, only 39 are presently known to have been rebuilt in the seventh (Stern 2001: 142; cf. Finkelstein 1994a).

Central to determining Sennacherib's destruction have been two factors: (1) the stratigraphy of Lachish and (2) the 1,200+ *lmlk* seal impressions found on storage jars in Jerusalem, Lachish and elsewhere. It has now been determined that Sennacherib's siege and destruction of Lachish is found in stratum III (see references in Grabbe [ed.] 2003: 6–8). Curiously, the siege and fall of Lachish are not mentioned in Sennacherib's inscriptions. As for the *lmlk* seals, their use was likely to have been over a rather short period of time because of the seal impressions of named officials on them and because *lmlk* jars were found in some sites (e.g., in Philistia) that did not come under Judah's control until Hezekiah's reign. If their production and use was thus over a very narrow time frame, they can be used to date particular events in the context of Hezekiah's rebellion and subsequent Assyrian attack (for information on the *lmlk* seals, see §5.1.2.6).

One of the central questions has been the way in which Sennacherib shut up Hezekiah 'like a bird in a cage'. Was this by the standard siege methods of surrounding the city by a siege mound? The difficulty has been excavation work in Jerusalem that found evidence of this period. A. van der Kooij (1986) examined two issues: (1) whether the Assyrian statements imply a siege and (b) whether there is any evidence for such a siege. Not strictly based on the archaeology, van der Kooij's argument involved the question of how to interpret the statements in the Assyrian inscriptions. He concluded that the wording of the Assyrian inscriptions did not envisage an actual siege. H. Tadmor (1985) also argued that Jerusalem did not come under siege; indeed, the Assyrian text also implies that it did not. More recently W. Mayer (1995; 2003) has similarly argued that the Assyrian army did not in fact set up siege works around Jerusalem directly. Yet just 4 km southwest of Jerusalem, Ramat Raḥel (Ḥirbet Šālih) seems to show evidence of a destruction of level VB which has 170 *lmlk* jar handles associated with it (J. P. Dessel 1997; Vaughn 1999a: 102–5).

The seventh century saw great changes, yet some were positive. Stern sums up things in this way:

The overall picture emerging from the excavations at the sites along the Judean Hill ridge appears to corroborate that in other parts of the Judean monarchy: a severe destruction followed Sennacherib's 701 BCE campaign at almost all sites, excluding Jerusalem. Between that date and the arrival of the Babylonians, the country enjoyed a period of rebuilding and relative prosperity. (Stern 2001: 163)

According to Stern (2001: 130–1), despite the widespread destruction by Sennacherib, the rebuilding process was quite rapid. A number of archaeologists put specific times on developments in the seventh century;

however, such a precise dating of events from archaeology can be disputed: 'Archaeologically, it is not easy to distinguish the finds of the early seventh century from those of the second half of that century' (Finkelstein and Silberman 2001: 265; cf. also 345–6). Being able to distinguish the archaeology from Manasseh's reign, as opposed to Josiah's, is not easy, though a number of finds go together to suggest that the main recovery of prosperity occurred early in the century (Finkelstein and Silberman 2001: 265–9; Stern 2001: 130–1).

Throughout the seventh century Jerusalem had no rivals, because most of those urban areas that approached it in size and importance in the eighth century had been destroyed by the Assyrians and had not been rebuilt (Tatum 1991: 141–2; Steiner 2001: 285; 2003a: 76–7). Even Lachish, which had its fortifications repaired, was only sparsely inhabited. There seems to be general agreement that in the Judaeian heartland the settlement area in general increased in the seventh century, though it was mainly in rural areas (including the desert fringe) or new towns or fortresses (Tatum 1991: 142; Finkelstein 1994a: 174–80). The increase in settlement area has been explained as the accommodation of those who were forced to leave the Shephelah when the Assyrians took it from Judaeian control. The situation varied somewhat in different parts of the country, as did the situation in the eighth century in comparison with that in the seventh. Not all of the country was devastated by the Assyrian army. The old area of Benjamin (now a part of Judah north of Jerusalem) had prospered in the late eighth century and continued to do so in the seventh, apparently having escaped (at least to some extent) the ravages of Sennacherib (Ofer 2001: 29).

The result was that Jerusalem was left as the only real urban centre in Judah in the seventh century (on the archaeology of Jerusalem, see in particular the essays in Vaughn and Killebrew [eds] 2003). Yet there is little evidence of the city's being an administrative centre: no public buildings were built in the seventh century, and the city seems mainly residential (Steiner 2001: 284). It may be that the city functioned primarily as a commercial and trading centre. This period also saw a huge expansion of Jerusalem, though exactly how early that began is disputed. Since the excavations in the Jewish Quarter in the 1970s, it is now widely accepted that this growth took in the western hill (Geva 2003). The expansion is thought by some to be a result of Sennacherib's invasion, but many now think that it began earlier, in the late eighth century BCE (Geva 2003: 203–7; Reich and Shukron 2003; Finkelstein 1994a: 175). One explanation for this unprecedented growth is that it was at least in part due to immigration from the northern kingdom after the conquest of Samaria and from the Shephelah after 701 BCE when a large part of it was removed from Judaeian control (Broshi 1974). The Broad Wall

was built about this time, apparently to protect this new western quarter. This growth is also evidenced by a new quarter on the eastern slope, in which a number of luxurious houses were constructed together with a new section of the city wall to the east to enclose this section within the fortifications. About this time there was also apparently expansion of settlement to the north, into an unfortified section of the city.

Conventional estimates put the size of all Jerusalem about 600 BCE at 600 dunams or a minimum of 6,000–7,000 inhabitants (Geva 2003: 206), but G. Barkay (as cited in Lipschits 2003: 327) thinks it was 900–1,000 dunams in size, which could mean a population of up to a maximum of 25,000 persons; however, such an estimate would take in even farms on the edge of the built-up area (Lipschits 2003: 327 n. 14). It has been argued that residential parts of the city to the north were damaged in Sennacherib's siege and remained deserted (Geva 2003: 207), but it seems unlikely that the Assyrian army actually encamped at Jerusalem or laid siege to the city in traditional fashion (cf. Grabbe [ed.] 2003: 8–10). Nevertheless, both the eastern extension and the northern expansion seem to have been abandoned before the fall of Jerusalem to the Babylonians, while the settlement on the western hill appears to have withdrawn to within the wall (Geva 2003: 207; Reich and Shukron 2003: 217).

One site has become controversial in recent years. 2 Kgs 20.20 states that Hezekiah made the 'pool' (הַבְּרִכָּה) and the 'conduit' (הַחֶעְלָה) and brought water into the city. The Siloam tunnel has long been thought to provide the evidence for the truth of this verse. J. Rogerson and P. R. Davies (1996) challenged this consensus and sought to redate the tunnel and its inscription to the time of the Maccabees. This identification has been widely rejected (e.g., Hendel 1996; Norin 1998); however, E. A. Knauf (2001b) has now argued for the reign of Manasseh as the time of its building. Thus, the question of whether the Siloam tunnel belongs to Hezekiah's time is far from settled, but it well fits other trends in the seventh century and would fall logically in the reign of Manasseh.

In the Judaeen hills south of Jerusalem, Ramat Rahel was a major centre. The site, with its citadel, was probably destroyed at the time of Sennacherib's invasion. A new citadel was built in the seventh century (stratum VA) and shows some Assyrian influence. It has often been interpreted as a royal palace, but why would the Judaeen kings build a palace so close to Jerusalem? N. Na'aman (2001) argues that it was Assyrian policy to construct emporia and centres of government near the capitals of vassal kingdoms. Ramat Rahel would be much better explained as an Assyrian building used as an administrative centre (though Judaeans would have been conscripted to do the actual work). As supporting evidence, the presence of Assyrian officials

can be proposed for a number of vassal kingdoms at this time, including Byblos, Tyre, Ashdod and Gaza.

According to this interpretation, economic considerations were a major factor, with Assyria actually competing with its vassals for revenues. No wonder anti-Assyrian rebellions broke out periodically! Assyria's relentless imperialism was severely damaging to its subjects. No exact dating for the destruction of the fortress is possible, and 587 is only one possibility. It may have been abandoned by the withdrawing Assyrians, or even destroyed by them as they left. Yet, other interpretations are possible. E. A. Knauf (2005: 170–1) argues that such building projects were often undertaken as propaganda for the ruling dynasty. It was not as if a royal palace so close to Jerusalem was *needed* but simply that it proclaimed the splendour of the king.

The Judaeen hills cover the general area from just south of Jerusalem on the north to Beersheba valley in the south, and from the Judaeen desert on the east to the descent to the coastal plain on the west, an area of approximately 900 km². The question of the population in the seventh century compared to the eighth is controversial (cf. Finkelstein 1994a: 174–5). The Judaeen hills survey (by A. Ofer; cf. Ofer 2001) found that from the mid-eleventh to the eighth century the population nearly doubled in each century, but the seventh century showed a decline compared to the eighth. Although the number of settlements was similar (86 in the seventh, compared to 88 in the eighth), the settled area was much less (70 hectares in the seventh, versus 90 in the eighth). This meant a decline in population from about 23,000 to about 17,000. Even if one slightly reinterprets the data for less of a contrast (from 84 to 74 hectares, or 21,000 to 18,500 inhabitants), there was still a marked fall off. This could be due to a variety of causes, but one obvious possibility is Sennacherib's invasion of 701 BCE.

I. Finkelstein, however, is not convinced of the contrast between the density of population in the Judaeen highlands between the eighth and seventh centuries (Finkelstein 1994a: 174–5). He notes that 'all major excavated sites in the Judaeen highlands were occupied in both the eighth and seventh centuries BCE' (1994a: 174). He makes several points (in addition to the one about the occupation of all major excavated sites in both the eighth and seventh centuries): that in the seventh century, possibly Tell en-Nasbeh and Gibeon, and certainly Ramat Rahel reached their peak, that a group of forts was established around Jerusalem, that a system of farmhouses was established around Jerusalem and Bethlehem, and that the Judahite population spread to nearby arid zones (Judaeen desert and Negev). Finkelstein argues that there may be reasons why the survey shows some differences between the two centuries, but he assumes that the Judaeen hills

south of Jerusalem were as densely populated in the seventh as the eighth century.

Sennacherib's invasion was hard on the Shephelah: the city of Lachish was one of the major cities besieged by the Assyrians (Uehlinger 2003), but others destroyed were Tell Beit Mirsim and Beth-Shemesh. Afterwards, the Assyrians appear to have removed a good portion of the Shephelah from Judaeen control. The late seventh and early sixth century saw a decrease of 70 per cent in built-up areas, mostly in unwallled villages and farmsteads and mostly in the eastern part of the region (Finkelstein 1994a: 172–4). About 85 per cent of the eighth-century sites were not resettled in the seventh. Archaeologists disagree about the case of Tell Beit Mirsim. E. Stern (2001: 149) states that the town was certainly rebuilt in the seventh century, before being destroyed by the Babylonians. However, Greenberg (1993: I, 180) notes: 'There is slight ceramic and stratigraphic evidence for a partial reoccupation of the site in the seventh or early sixth century BCE.' The only site that grew was Tell Migne (usually identified with ancient Ekron), but this area had generally been outside Judah. Tell Batash (Timnah) was also apparently outside Judah but likely under the control of Ekron (the pottery is mixed but Philistine predominates).

One of the interesting phenomena of the seventh century was the growth in settlement in the arid regions: the Negev and the eastern deserts. En-Gedi may have been settled in the eighth. In any event, both it and Jericho grew considerably in the second half of the seventh century, with an unprecedented amount of settlement (59 sites) between the two sites (Lipschits 2003: 338–9). Beersheba was abandoned at this time, but new sites were also established at Tel Masos, Ḥorvat 'Uza, Ḥorvat Radom, Tel 'Ira, and Aroer (Finkelstein 1994a: 175–6).

Arad has often been discussed in connection with the reign of Josiah, but it is a site about which prominent archaeologists have come to some significantly different conclusions, mainly because the excavator Y. Aharoni was not able to publish a full report before his death (see especially the summary in Manor and Heron 1992). The original interpretation was that a shrine persisted through layers XI–IX, consisting of a courtyard with a large altar, a broadroom and a small inner room apparently with a stela (מצבה) and two incense altars. Some alterations were made over time, but in stratum VIII some significant changes were made in the temple area: it was dismantled and much of it covered in a metre-thick layer of earth. This activity has been associated with the cult reforms of Hezekiah. Nevertheless, the stratigraphy, the dating of the strata, and the possible historical events with which they are to be associated are all disputed. D. Ussishkin (1988), for example, interpreted matters rather differently (though depending on the

preliminary publications and interviews with some of the excavators). He noted (with others) that the homogeneity of the pottery from strata X–VIII suggests a relatively short period of time, and these layers may represent only different stages of a single building phase. The shrine, which some had dated even as early as stratum XI, was in fact not built until stratum VII.

Now, a new complete stratigraphical examination conducted in 1995–96 has led to a re-evaluation, with differences from that of the original excavator and the many studies built on it (Herzog 2001: 156–78; cf. Uehlinger 2005). There were particular problems in relating the temple complex to the stratigraphy by the earlier excavators, partly because of the ‘biblical archaeology’ paradigm embraced by the excavation team (among whom Herzog includes himself). Stratum VII elsewhere in the site was 2.5 m above the temple floor, showing that the temple was earlier than that (contra Ussishkin). The temple began in stratum X (not XI, as widely believed) or the eighth century, with a second phase of construction in stratum IX. The temple was not destroyed by fire but was dismantled in stratum VIII (late eighth century), which Herzog attributes to Hezekiah. The question is, though, whether the dismantling was done to destroy the temple or, on the contrary, to protect the pillars and so on, for future use (cf. Uehlinger 2005: 287–92; Niehr 1995: 35; Ussishkin 1988).

This settlement in the eastern and southern arid regions was short-lived for the most part, however, collapsing at the end of the Iron Age (Lipschits 2003: 334–7). In the eastern desert sites, there is no evidence of physical destruction but gradual abandonment. In the Negev, a number of the fortresses show signs of destruction (including Horvat ‘Uza and Arad and the settlement sites of Tel ‘Ira and Tel Malḥata). Other sites appear to have been abandoned (e.g., Horvat Radum). It seems that the decline came about primarily because the areas of Judah which the arid regions depended on for military and economic support had themselves been destroyed by the Babylonians. The inhabitants of the settlements gradually withdrew because they had no other choice.

The archaeology also provides data about aspects of the economy, though this often has to be put together with other data. The Edomite plateau gained a significant population at this time, and there are indications of contact with Arabia (Finkelstein 1994a: 177–80; Finkelstein and Silberman 2001: 267–9). This could suggest the place of Judah in a trade route from Arabia. We know from Assyrian sources that Gaza was an important trading centre for the Assyrians. The seventh-century forts at Qadesh-Barnea and Haseva (both probably outside Judah; see below) may have been built with the protection of this trade in mind. There is also evidence of a major olive-oil production centre at Tel Miqne (Ekron). This was an old Philistine city and not part of

Judah; however, the olives could not be grown locally, which leads to the inference that they would most likely have been imported from the Samaritan and Judean highlands.

One feature of the archaeological data that particularly stands out is the sudden increase in the quantity of written objects preserved from the seventh century: seals/bullae, ostraca and inscribed weights (Finkelstein and Silberman 2001: 270, 281, 284; Stern 2001: 169–200). This phenomenon is apparently not found in the neighbouring Assyrian provinces or vassal states. It thus appears to be a genuine increase in the production of written objects in Judah at the time and not just an impression created by the accident of discovery. A number of conclusions have been drawn from this fact, not all of them justified (§5.2.4; §3.2.5).

Attempts have been made to determine the borders of Judah from archaeological data. Several sites have been put forward as an indication of an expansion of the territory of Judah during the reign of Josiah. The arguments are only in part based on archaeology, but the archaeology of the sites is important: Megiddo, Mešad Hashavyahu, Haseva and Qadesh-Barnea. With regard to Megiddo, no Judean artifacts have been found from the seventh century (stratum II). A consideration of the historical situation suggests that it was more likely under Egyptian control (cf. Na'aman 1991: 51–2), but no Egyptian artifacts have been found, either (Finkelstein and Silberman 2001: 350). This might be the case if the occupation was brief, whether by Josiah or the Egyptians, but the fact is that nothing in the archaeology connects Megiddo with Josiah. With regard to the fortresses at Haseva and Qadesh-Barnea (cf. Cohen and Yisrael 1995; Cohen 1981; 1997), the archaeology is ambiguous. They seem to be of Assyrian construction (Na'aman 1991: 48). Although Judean artifacts have been found at Qadesh-Barnea (Judean inscribed weights, pottery vessels, Hebrew ostraca), the material culture is mixed (Kletter 1999: 42). A number of ostraca were found at Qadesh-Barnea, including two with Hebrew writing, some written in Egyptian hieratic, and at least two with a mixture of Hebrew and Egyptian. Once again, the connection with Josiah comes primarily from considerations other than the archaeology. Na'aman (1991: 48–9), on the other hand, suggests that the best way to explain the archaeology is that as the Assyrians withdrew, the Egyptians took over.

Based on an ostrakon, as well as the pottery, Mešad Hashavyahu has been a prime part of the argument for Josiah's expansion, since it is not only well outside the traditional territory of Judah but also on the coast, which could suggest an effort by Judah to obtain its own outlet to the sea. The excavations of this settlement on the coast have only recently been published (Fantalkin 2001). The settlement was very short-lived, only a couple of

decades or so. From archaeological evidence alone, it was either late seventh century or early sixth. There is actually no direct evidence for supposing it to be linked with Josiah. A large amount of eastern Greek pottery has suggested that a large portion of the population was Greek, but the names in the published ostrakon are Hebrew, three with the name Yahu and one with Baal (Gogel 1998: 423–4). One theory suggests that it was a Greek trading colony, but the location and the finds do not support this. It is more likely that it was a settlement of Greek mercenaries, though also with some Judaeans in various capacities (Na'aman 1991: 44–6; Fantalkin 2001: 139–47; Finkelstein and Silberman 2001: 350–1). If so, they were probably in Egyptian service (Psammetichus I or possibly even Necho II). The Egyptians controlled this area between the withdrawal of the Assyrians and the coming of the Babylonians. For it to be settled by mercenaries in the service of Judah is not very credible. Judaeans control is even less likely if the date of Mešad Ḥašavyahu is lowered to 600 BCE (Kletter 1999: 42). Stern (2001: 140–2) objects, pointing to the lack of Egyptian remains; however, the site also lacks characteristic Judaeans remains (e.g., rosette seal impressions), and the short time of occupation might have left no characteristic Egyptian remains (though one cooking-pot lid is possibly Egyptian). The site could have been destroyed by the Babylonians in 604 BCE, along with Ashkelon and other areas on the coast.

Also as an indication of borders, a number of artifacts have been considered as of potential help (Kletter 1999). These include the following. (1) 'Judaeans pillar figurines'. Most of these are from the eighth and seventh centuries. They show differences from pillar figurines of other areas (such as Israel and Transjordan). It has been suggested that these represent Asherah, though any direct proof is lacking, and little evidence exists of a relation to the cult. But none was apparently broken deliberately. (2) Inscribed scale weights. The main period of use was the seventh century, though some are as early as the eighth. They are thus not an invention of Josiah. Most likely common weights rather than royal, they may represent private trade relations rather than public administration. (3) Horse and rider figurines. Most of these are from the eighth century, but some are dated to the seventh. Of the five types, the first type is definitely Judaeans, while types 3 and 4 are Phoenician and Transjordanian, which means that the Judaeans type is easily distinguishable from neighbouring areas. (4) The rosette stamps (on these, see §5.1.2.6). Their distribution leaves no doubt of their Judaeans identification, but since hardly any are found in the central hill country of Negev, it makes it hard to use them for political borders.

Kletter (1999: 40–3) comes to several conclusions about the artifacts in relation to Judah's possible borders. He notes that the overwhelming majority

of artifacts are found in the Judaeen heartland: 96 per cent of Judaeen pillar figures, 98 per cent of the horse and rider figures, 96 per cent of the rosette seal impressions, and 75 per cent of the Judaeen inscribed weights. Various explanations have been given for those outside the heartland, such as trade, but only the western Shephelah shows a meaningful concentration of Judaeen artifacts. Archaeology cannot pinpoint accurate borders nor establish the political affiliation of single sites. But the finds fit more or less with the Judaeen heartland and do not indicate large-scale expansion. They cannot prove or refute the possibility that Josiah or another king lost or gained small areas for short durations. Nevertheless, artifacts are varied and also of general value for political borders: some relate to trade, some to religious beliefs.

O. Lipschits (2005: esp. 185–271) has provided the definitive account of the archaeology of Judah in the sixth century BCE. The local pottery assemblages show an unbroken material cultural tradition in Judah from the end of the Iron Age into the Persian period. The settlement continuity from Babylonian to Persian rule has made it quite difficult to distinguish these periods. The distinction between Lachish strata 3 and 2 provides the chronological and stratigraphic key. It is only in the mid-fifth century that the pottery repertoires begin to include types and forms considered typical of the Persian-period local culture. To date all destruction layers to 587/586 in Judah is to depend on the text, not archaeology: the notion of a demographic and material-culture gap in the sixth century BCE goes too far. We find considerable variation across the region. In Judah there was a sharp decline in urban life but continuity in rural settlements in the highlands, particularly between Hebron and Benjamin. As we might expect from the text, Jerusalem and the cities of the west were destroyed, following which we have a collapse in the east (the Judaeen desert, the Jordan Valley, the Dead Sea region) and south (the south Shephelah and the Negev). Benjamin and the Judaeen highland, which apparently escaped the Babylonian destruction, contained most of the population. As for Jerusalem itself, signs of the Babylonian destruction are found throughout the city. Surprisingly, there is no evidence of settlement from then until the middle of the Persian period: Jerusalem seems to have been uninhabited during the four decades or so of the ‘exilic period’. The capital had apparently been moved to Tell en-Nasbeh (Mizpah), and Benjamin was where most people in the province now lived.

5.1.1.1 Conclusions with Regard to Archaeology

1. The two important horizons in the stratigraphy of this time are the destructions by Sennacherib and Nebuchadnezzar. They are helpful in

defining the layers relating to the seventh and early sixth century. The stratigraphy and dating at Lachish is extremely important, and much of the interpretation of other sites depends on their relation to the finds at Lachish.

2. It is difficult, however, to distinguish between the early seventh century and the second half of the century. Very specific dating by archaeology alone is not necessarily possible. This means that assertions about developments in the reign of Manasseh versus that of Josiah or later kings (some quoted in the discussion above) need to be looked at carefully.
3. The area of Judah where the *lmlk* seals are found and where destruction seems to have taken place in relation to the events of 701 BCE is a fairly well defined area (see especially Na'aman 1986a). The main cities of defence were those in the Judaeen hill country and the Shephelah, with Hebron as the main hub. Most of them show evidence both of the stamped jar handles and attack by the Assyrians. The distribution of finds also has interesting affinities with the list of fortified cities in 2 Chron. 11.5-10, though the exact relationship has been much debated (Na'aman 1986a; 1988a; Garfinkel 1988; Hobbs 1994; Ben Zvi 1997).
4. There is so far no archaeological or other evidence that Jerusalem was invested by the Assyrian army in the sense of being surrounded by a siege mound and having an Assyrian army camped outside its wall. It was 'shut up' by having the various communication routes blocked, but the nearest evidence of the Assyrian army is presently at Ramat Raḥel, 4 km from Jerusalem.
5. Jerusalem's position was considerably enhanced in the seventh century. Although the question of whether some of the main eighth-century sites were settled seems to be answered differently by different interpreters, the size and dominance of Jerusalem appears to have been many times greater than any other cities. The expansion had already begun in the eighth century, but because Sennacherib destroyed most other towns in Judah, Jerusalem was left without a rival. This dominance does not appear to have come about because of greater centralization of the administration, however, since the character of Jerusalem is that of a residential city. The Siloam tunnel, usually assumed to have been built during the time of Hezekiah, may well belong to the time of Manasseh.
6. The borders of Judah do not appear to have enlarged under Josiah's rule as is sometimes alleged. On the contrary, at the beginning of the seventh century, there was an immediate loss of territory in that much

of the Shephelah was taken away. Perhaps to make up for this loss, evidence exists that settlements extended into some of the arid zones of the east and the south. There is some indication of a boundary as far north as Bethel, but the areas of Samaria, the Galilee, the coastal plain or Transjordan do not show any signs of Judaeen expansion (Na'aman 1991). Allegations that the borders of Judah took in areas as far as Mešad Hashavyahu and even Megiddo do not appear to have much support in the archaeological record and would have been brief, if at all. Despite claims to the contrary, there seems little archaeological evidence of either political or religious centralization in the seventh century. The evidence of cult suppression claimed for Arad is ambiguous.

7. The archaeology provides some evidence of economic developments. Population growth in the Negev area, objects allegedly inscribed with Old South Arabian inscriptions (though Professor Knauf has pointed out to me that the South Arabian connection is uncertain) and forts in the south of the country (such as Qadesh-Barnea) may be evidence of trade through the region of Judah. There is also evidence of olive-oil production on a major scale at Ekron (Tel Miqre), though the olives themselves would have had to come from elsewhere, including the Judaeen highlands.
8. The much greater number of ostraca, inscribed seals and other written objects seems to be evidence of the development of the bureaucracy. Whether it indicates anything else, such as an increase in literacy is a matter of debate (§3.2.5).
9. The Babylonian destruction of Jerusalem and the surrounding area caused a collapse further afield, in the east and south. But Benjamin had not suffered from the Babylonian invasion and became the centre of the province for the next century. All the evidence suggests that Jerusalem itself remained uninhabited from 587/586 to the beginning of Persian rule.

5.1.2 Palestinian Inscriptions

5.1.2.1 The Adon Papyrus

The date and the sender of this document have not been preserved; however, a Demotic notation on the back of the document has been interpreted as a reference to Ekron (CoS III, 132–3). The papyrus is generally dated to the late seventh century, but this is only an educated guess. In it Adon, king of a site now lost from the manuscript, calls on the Pharaoh for help.

5.1.2.2 Mešad Hashavyahu

The following translation of ostracon 1 is from Gogel:

- 1 May the official, my lord, hear
 - 2 the plea of his servant. Your servant
 - 3 is working at the harvest ...
 - 6 When your [se]rvant had finished his reaping and had stored
 - 7 it a few days ago, Hoshayahu ben Shabay came
 - 8 and took your servant's garment. When I had finished
 - 9 my reaping, at that time, a few days ago, he took your servant's garment.
 - 10 All my companions will testify for me, all who were reaping with me
 - in the heat of
 - 11 the sun – they will testify for me that this is true. I am guiltless of an
 - 12 in[fraction]. (So) please return] my garment. If the official does
 - (= you do) not consider it an obligation to retur[n]
 - 13 [your] ser[vant's garment, then hav]e pi[ty] upon him
 - 14 [and re]turn your [se]rvant's [garment]. You must not remain silent.
- (Gogel 1998: 423–4)

5.1.2.3 Arad Ostraca

The Arad archive is associated with a man named Eliashib who seems to have been commander of the fortress. Ten of the ostraca speak about the 'Kittim', often with regard to distributing rations to them (## 1, 2, 4, 5, 7, 8, 10, 11, 14, 17). These are usually interpreted as Greek mercenaries, but which king they served is still a moot point. The following quotation is from Arad 18:

- 1 To my lord Elyashib.
- 2 May Yhwh concern
- 3 himself with your well-being. And now,
- 4 give Shemaryahu
- 5 one *letek*-measure, and to the Qerosite
- 6 give one *homer*-measure. Regarding
- 7 the matter about which you
- 8 gave me orders: everything is fine:
- 9 he is staying in
- 10 the house (temple) of Yhwh. (Gogel 1998: 390–1)

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5.1.2.4 Lachish Letters

An important collection of ostraca was found at Lachish. Some of these seem to have been correspondence (perhaps military) in the period not long before the capture of Jerusalem in 587/586. Lachish 4 says:

- 1 May Yhw[h] cause my [lord] to hear
- 2 good tidings at this time!...
- 9 For if he [co]me[s around] during the morning tour
- 10 he will know that we
- 11 are watching the Lachish (fire-)signals, according to all the signs which my lord
- 12 gave us, for we cannot see
- 13 Azeqah. (Gogel 1998: 417)

5.1.2.5 The Ashyahu Ostrakon

An ostrakon published in 1996 has been associated with the reign of Josiah (Bordreuil, Israel and Pardee 1996; 1998). The ostrakon was obtained on the antiquities market and is of unknown provenance. More recently I. Eph'al and J. Naveh (1998) have outlined their reasons for 'hesitations regarding the authenticity' of this document (plus another ostrakon published at the same time).

5.1.2.6 Seals and Bullae

One of the valuable sources of information for the eighth and seventh centuries BCE is the seal impressions (and occasionally the actual seals themselves) from both public officials and private individuals. A number of these have been found in proper archaeological contexts, but many were obtained on the antiquities market. This always leaves open the possibility that some of the seals and impressions cited in scholarly literature are counterfeits (§1.2.3.7). Fortunately, we have a number of provenanced seal impressions.

5.1.2.6.1 *Lmlk and Rosette Seals and Impressions*

The *lmlk* seal impressions get their name because they all have the Hebrew word *lmlk* ('to/for the king'), either by itself or with one or more names (Ussishkin 1977; Na'aman 1979; 1986a; Tushingham 1992; Barkay and

Vaughn 1996; Vaughn 1999a). More than 1,200 *lmlk* seal impressions have been found on storage jars in Jerusalem, Lachish and elsewhere. They tended to be present on the four-handled jars with certain physical characteristics in common, and to occur associated with one of two particular emblems: a four-winged type (in two varieties) and a two-winged type. In some cases, 'private seals' are found on jars which also have the *lmlk* seal impressions. On various of the *lmlk* seal impressions are found four names which seem to be place names: Hebron, *mmšt*, Socoh and Ziph. The jars all seem to have been produced in a single pottery centre (Mommensen, Perlman and Yellin 1984; Na'aman 1986a: 16–17), and examples of the four-winged seal impressions have been found in the same sealed destruction layer as the two-winged (Aharoni 1979: 394–400; 1982: 254–64; Ussishkin 1977: 54–7; Na'aman 1979: 70–1). The conclusion is that the jars were produced by Hezekiah as a storage unit important in trade but also ideal for stockpiling liquid foodstuffs (especially wine) in anticipation of invasion (Vaughn 1999a: 152–7). Their use was likely to have been over a rather short period of time because of the seal impressions of named officials on them, which would not have been used for more than a few decades, and because *lmlk* jars were found in some sites (e.g., in Philistia) that did not come under Judah's control until Hezekiah's reign.

The old view was that they continued to be used to the fall of the kingdom of Judah, but more recently interpreters have seen them as confined to the reign of Hezekiah. A. Mazar (1993: 455–8) and more recently E. Stern (2001: 174–8) have argued, however, that a few continued to be used for some decades after Hezekiah's death. Mazar seems to be suggesting not that more jars were manufactured but that jars already in use continued to be around and utilized until their working life came to an end; Stern, however, puts forward the view that the jars continued to be manufactured, with only the seal types changing (to the rosette and concentric circle), though at least one *lmlk* seal was used as late as Josiah's reign. The idea that *lmlk* jars continued to be produced has been strongly challenged by Vaughn (1999a: 106–9). Because Jerusalem would have had many such jars but was not conquered, it is inevitable that some would have been used after Hezekiah's time. But of the other sites adduced (including Arad) only Khirbet Shilḥah has a *lmlk* jar handle from a clear seventh-century context (and the jar itself was not restorable).

It seems to be generally accepted that the rosette seal and the concentric circles are developed stylized forms of the four-winged and two-winged seal impressions (Na'aman 1991: 31–3; Stern 2001: 176–8). The presence of these is an indication of a seventh-century archaeological layer, much as the *lmlk* seals are of the late eighth. Some want to date the rosette seal impressions

specifically to the reign of Josiah or Jehoiakim, but as Kletter (1999: 34–8) argues, there is no reason for such a narrow dating. It often seems to be taken for granted that they are royal, but the rosette motif was widespread in the ancient Near East, and there are many examples of non-royal use. Unlike the *lmlk* seal, which is dominant in the eighth, the rosette is only one of a number of types in the seventh century.

5.1.2.6.2 Other Seals and Seal Impressions

This section gives a somewhat abbreviated summary; for more details, see Grabbe (2006a). The bullae in Y. Shiloh's excavations in Jerusalem (Shiloh 1986; Shiloh and Tarler 1986; Shoham 1994; 2000; Avigad and Sass 1997; Mykytiuk 2004: 139–47) were found in stratum 10B which appears to cover the last few decades of the kingdom of Judah and ends in 587/586.

- 'Gemaryahu son of Shaphan' (לגמריהו בן שפן): see the discussion in Mykytiuk 2004: 139–47), which the stratigraphy would date to about 630–586 BCE, a dating not contradicted by the epigraphy. This is the same name as Gemariah son of Shaphan the scribe who has a chamber in the temple precincts (Jer. 36.10) and is an official in the palace (Jer. 36.12). The name Gemariah occurs in six other provenanced inscriptions but Shaphan in only one (Mykytiuk 2004: 142 n. 133). The one missing ingredient in the seal inscription is the title 'scribe' or something similar; nevertheless, the possibility of identification of the individual in this seal with one in Jeremiah is relatively high (Mykytiuk 2004: 146).
- 'Gedalyahu who is over the house' (לגדליהו אשר על הבית) [Gogel 1998: 487 (Lachish seal 6); *AHI*: 100.149; Becking 1997: 75–8)]. This has been widely identified with the Gedaliah who was made governor (king?) of Judah after the destruction of Jerusalem in 587/586; however, this identification has been labelled as 'disqualified' because if the script is dated to the mid-seventh century (as some do), it would be too early for the individual in Jeremiah 40–41 (Mykytiuk 2004: 235). Whether the alleged palaeographical dating is so accurate as to rule out the early sixth century is a question (cf. Vaughn 1999b), but the existence of another official called Gedaliah son of Pashhur (Jer. 38.61) shows that the name was a relatively common one. Another seal with a similar inscription (לגדליהו עבד המלך) 'Gedalyahu the servant of the king' is in identical script but was obtained as part of the 'Burnt Archive' published by Avigad (1986: 24–5). The lack of a proper provenance is a

major difficulty. Thus, one has to conclude with B. Becking (1997: 78) that the possibility that this is from a seal belonging to the Gedaliah of Jeremiah 40–41 is relatively low.

- 'Milkomor the servant of Baalisha' (למלכמער עבד בעלישע): Herr 1985; Becking 1993; 1997). The name 'Baalish' (בעליש) was unique to Jer. 40.14 until this seal impression was found. Since a king of Ammonites with a name of this sort is so rare, I agree with Becking (1997: 82) that the possibility of an identification is high, despite the differences between the names.
- 'Jaazaniah the servant of the king' (יאזניה עבד המלך): *AHI*: 100.069; Avigad and Sass 1997: no. 8). As well as the inscription, it contains the image of a fighting cockerel. A Jaazaniah son of the Maachite (יאזניהו בן מהכתי) was associated with Gedaliah at Mizpah after the fall of Jerusalem (2 Kgs 25.23). The chances are that he was some sort of royal official before the Babylonian siege. I would consider the identification as of moderate probability. Finally, a seal found in the Shiloh excavations in Jerusalem has the name 'Azariah son of Hilkiah' (אזריהו בן חלקיה): Gogel 1998: 485 (Jerusalem Bulla 27); *AHI*: 100.827; Mykytiuk 2004: 149–52). The high priest Hilkiah (2 Kgs 22.4–14; 23.4) had a son named Azariah (1 Chron. 6.13; 9.11; Ezra 7.1). I would put the identification as moderately high.
- 'Belonging to Eliaqim servant of Yochan'; on the seal impressions with this inscription, see §1.2.3.6.

We now come to the unprovenanced bullae. One set is that published by N. Avigad as 'remnants of a burnt archive from the time of Jeremiah'. Unfortunately, they were obtained on the antiquities market and are of unknown provenance. Avigad asserted that there 'was no reason to suspect their authenticity, and I seriously doubt whether it would be possible to forge such burnt and damaged bullae' (1986: 13). The question of forgery cannot be so lightly dismissed: no scientific tests appear to have been applied to these bullae. If they are authentic, the biblical parallels are striking – which is precisely why their authenticity needs to be scrutinized. In some ways, these seal impressions in the 'Burnt Archive' are superficially more interesting than those excavated by Shiloh and other archaeologists because of their biblical parallels; however, this greater incidence of biblical names in itself might arouse some suspicions: are they 'too good to be true'? It illustrates the problems of working with material bought on the antiquities market.

- 'Berekyahu son of Neriah the scribe' (לברכיהו בן נריהו הספר): Avigad 1978; 1986: 28–9; Avigad and Sass 1997: #417; Mykytiuk 2004:

188–90). If the seal impression is authentic, the parallel to the biblical Baruch would be impressive (but see below).

- ‘Jerahmeel son of the king’ (לִירְחֵמֶאֱל בֶּן הַמֶּלֶךְ): Avigad 1978; 1986: 27–8; Avigad and Sass 1997: #414; Mykytiuk 2004: 191–6). According to Jer. 36.26 an individual with this name and title was one of the officials sent by Jehoiakim to arrest Jeremiah and Baruch.
- ‘Elishama servant of the king’ (לְאִישָׁמַע [עַבְדֵּי] הַמֶּלֶךְ). Elishama the scribe is one of the king’s officials giving a hearing to Jeremiah’s prophecies (Jer. 36.12); he could also be designated as ‘servant of the king’ because of his office.
- ‘Ishmael son of the king’ (לִישְׁמַעֲאֵל בֶּן הַמֶּלֶךְ): Barkay 1993; Becking 1997: 78–80). The dating to about 600 BCE is said to be consistent with the palaeography, but there is no confirmation from an archaeological context. As A. G. Vaughn’s detailed investigation has shown, the palaeographic dating of seals is far from precise, with only half a dozen letters being diagnostic (1999b). The title ‘son of the king’ is found on a number of seals and impressions. Whether it is a literal son of the king or only a ‘title of an official’ is not certain (see Mykytiuk [2004: 194 n. 109] for a bibliography of discussion on the subject). Ishmael was apparently in some way a member of the royal family (2 Kgs 25.25; Jer. 41.1). Becking’s estimation is that this had a moderate probability of being authentically identified with the figure of Jer. 40 (on a scale of 1 to 10, he puts it at 5. Similarly, Mykytiuk rates it a 2 on a scale of 1 to 3 or ‘reasonable but uncertain’ [2004: 235]. I might put it a bit lower because the name was not infrequent, but I think this evaluation is reasonable (though the question of authenticity has not been seriously investigated, as far as I know).

An important question is the relationship of the iconic to the aniconic seals. It was observed several decades ago that there seems to have been a move from iconic to aniconic seals (or seals with only a small amount of ornamental decoration) in Judah between the eighth and seventh centuries; (Uehlinger 1993: 278–81 conveniently catalogues some of the past discussion; see also Keel and Uehlinger 1998: ch. 8, esp. 354–60). This trend toward aniconic seals does seem to be genuine, as far as extant seals are concerned, though Uehlinger (1993: 287–8) has shown that the matter is more complicated than sometimes represented. Even though there are other possible factors (e.g., increasing literacy), the influence of a growing ‘Deuteronomic movement’ is plausible. Perhaps more significant is the loss of astral images. The coming of the Assyrians in the eighth and seventh centuries saw a major increase in the use of Aramaean-influenced astral imagery on seals (see Uehlinger 1995:

65–7; 2005; Keel and Uehlinger 1998: ch. 8). The change on Judaeans seals is not so apparent, but when they are seen in the context of other seals in the region, the astralization of the iconography is readily identified. This imagery disappears from Judahite seals by the late seventh century.

5.1.3 Assyrian Sources

We are fortunate in the amount of Assyrian material we possess which is one of the main sources for the history of the ancient Near East at this time. The relevant inscriptions are catalogued below under the particular Assyrian king who was responsible for them.

5.1.3.1 Sargon II (721–705 BCE)

The most significant inscriptions of Sargon II are those that suggest he conquered Jerusalem and exiled its inhabitants (Tadmor 1958b: 33–40). Then in 716 he settled a colony of Assyrians on ‘the brook of Egypt’, the Egyptian border (Tadmor 1958b: 35, 77–8). In 713–712 Ashdod revolted and was conquered and turned into a colony (1958b: 79–80; *ANET*: 286–7). The biblical text suggests that Egypt was the instigator of this revolt (Isaiah 18), and Judah may even have been tempted to join (Isaiah 20). Wisely, though, she seems to have remained aloof and did not revolt until Sennacherib came to the throne.

5.1.3.2 Sennacherib (705–681 BCE)

The recent excavations at Lachish have not only established a good deal about the history of Lachish but, together with the remarkable reliefs from Sennacherib’s palace, provide a unique insight into an ancient historical event (Ussishkin [ed.] 2004; Ussishkin 1980; 1982). The pictorial depiction of the siege and conquest of Lachish is found in Sennacherib’s palace in the new capital of Assyria at Nineveh. These wall reliefs were already found and described in the early days of Assyriology (Layard 1853a; 1853b).

Scholarly work on the Sennacherib inscriptions has made an important contribution to progress in historical understanding (Levine 1983). The various inscriptions were collected in an edition with English translation by D. D. Luckenbill (1924), and then by R. Borger (1979). One of the significant recent additions to the textual repertory was made by N. Na’aman (1974;

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1979), who demonstrated that fragments already known actually belonged together. The most recent study is that of E. Frahm (1997), while W. Mayer (2003) has now rechecked the inscriptions and produced an edition and English translation. A study of the reliefs, with impressive drawings by Judith Dekel, was made by D. Ussishkin (1982). A critique of Ussishkin and others, with an extensive new interpretation of the reliefs, has been given by C. Uehlinger (2003). These and the archaeology confirm the savage attack on and destruction of Lachish hinted at in 2 Kgs 18.14, 17.

5.1.3.3 Esarhaddon (681–669 BCE)

Sennacherib was assassinated in 681 BCE and was succeeded by the crown prince Esarhaddon. Most of the royal inscriptions for this ruler have been collected in Borger (1967, in transliteration and German translation).

I called up the kings of the country Hatti and (of the region) on the other side of the river (Euphrates) (to wit): Ba'lu, king of Tyre, Manasseh (*Me-na-si-i*), king of Judah (*Ia-ú-di*) Qaushgabri, king of Edom, Musuri, king of Moab, Sil-Bel, king of Gaza, Metinti, king of Ashkelon, Ikausu, king of Ekron, Milkiasbapa, king of Byblos, Matanba'al, king of Arvad, Abiba'al, king of Samsimuruna, Puduil, king of Beth-Ammon, Ahimilki, king of Ashdod – 12 kings from the seacoast ... 10 kings from Cyprus (*Iadnana*) amidst the sea, together 22 kings of Hatti, the seashore and the islands; all these I sent out and made them transport under terrible difficulties, to Nineveh, the town (where I exercise) my rulership, as building material for my palace: big logs, long beams (and) thin boards from cedar and pine trees, products of the Sirara and Lebanon (*Lab-na-na*) mountains, which had grown for a long time into tall and strong timber, (also) from their quarries (lit.: place of creation) in the mountains, statues of protective deities (lit.: of Lamassû and Shêdu). (ANET 291)

5.1.3.4 Ashurbanipal (669–627 BCE)

Ashurbanipal's reign was in many ways the height of the Assyrian empire, but it had come to the edge of a precipice. The internal chronology of his reign is one of the most uncertain in Assyrian history: the last of the annals cease about 639, which leaves a gap in information until the *Nabopolassar Chronicle* takes up about 626 BCE.

In my first campaign I marched against Egypt (Magan) and Ethiopia (Meluhha). Tirhakah (*Tarqû*), king of Egypt (*Muṣur*) and Nubia (*Kûsu*), whom Esarhaddon, king of Assyria, my own father, had defeated and in whose country he (Esarhaddon) had ruled, this (same) Tirhakah forgot the might of Ashur, Ishtar and the (other) great gods, my lords, and put his trust upon his own power. He turned against the kings (and) regents whom my own father had appointed in Egypt ... (Then) I called up my mighty armed forces which Ashur and Ishtar have entrusted to me and took the shortest (lit.: straight) road to Egypt (*Muṣur*) and Nubia. During my march (to Egypt) 22 kings from the seashore, the islands and the mainland ...

Ba'al, king of Tyre, Manasseh (*Mi-in-si-e*), king of Judah (*Ia-û-di*), Qaushgabri, king of Edom ... together 12 kings from the seashore, the islands and the mainland; servants who belong to me, brought heavy gifts (*tâmartu*) to me and kissed my feet. I made these kings accompany my army over the land – as well as (over) the sea-route with their armed forces and their ships (respectively)... Afterwards, (however), all the kings whom I had appointed broke the oaths (sworn to) me, did not keep the agreements sworn by the great gods, forgot that I had treated them mildly and conceived an evil (plot)... They continued to scheme against the Assyrian army, the forces (upon which) my rule (was based), (and) which I had stationed (in Egypt) for their own support. (But) my officers heard about these matters, seized their mounted messengers with their messages and (thus) learned about their rebellious doings. They arrested these kings and put their hands and feet in iron cuffs and fetters. The (consequences of the broken) oaths (sworn) by Ashur, the king of the gods, befell them. I called to account those who had sinned against the oath (sworn by) the great gods (and those) whom I had treated (before) with clemency... Those kings who had repeatedly schemed, they brought alive to me to Nineveh. From all of them, I had only mercy upon Necho and granted him life. I made (a treaty) with him (protected by) oaths which greatly surpassed (those of the former treaty)... I returned to him Saïs as residence (the place) where my own father had appointed him king. (ANET 294–5)

Two minas of gold from the inhabitants of Bit-Ammon (^{mat}*Bît-Am-man-na-a-a*); one mina of gold from the inhabitants of Moab (^{mat}*Mu'-ba-a-a*); ten minas of silver from the inhabitants of Judah (^{mat}*Ia-û-da-a-a*); [...mi]nas of silver from the inhabitants of [Edom] (^{mat}*[U-du-ma]-a-a*). (ANET 301)

5.1.4 Babylonian Sources

The fall of Assyria happened quickly and for unknown reasons since the data are missing at important points. Sin-sharra-ishkun was king for much of the time from 627–612 when Nineveh succumbed to a coalition

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of Babylonians and Medes under Nabopolassar. A remnant of the empire continued in the west for a few more years under Ashur-uballit II who ruled in Harran.

5.1.4.1 Nabopolassar (626–605 BCE)

He was the ruler of Babylon who allied with the Medes to bring down the Assyrian empire and was founder of the short-lived Neo-Babylonian empire. We know a good deal about him because of the *Nabopolassar Chronicle*.

[The fourteenth year]: The king of Akkad mustered his army [and marched to...] The king of Umman-manda [*marched*] towards the king of Akkad [...]...they met one another. [40] [The k]ing of Akkad...[...Cy]axares...brought across and they marched along the bank of the Tigris. [...]they encamp]ed against Nineveh. From the month Sivan until the month Ab – for three [months –...]...they subjected the city to a heavy siege. [On the Nth day] of the month Ab [...] they inflicted a major [defeat upon a g]reat [*people*]. At that time Sin-sharra-ishkun, king of Assyria, [*died*]...[...][45] They carried off the vast booty of the city and the temple (and) [turned] the city into a ruin heap... [On the Nth day of the] month [...*Ashur-uballit (II)*] [50] ascended the throne in Harran to rule Assyria. (Grayson 1976: 94–5 = *Chronicle* 3: 38)

5.1.4.2 Nebuchadnezzar II (605–562 BCE)

This was one of the great kings in history, even if the biblical portrait has made him notorious. Unfortunately, we have little information after 594 BCE when *Babylonian Chronicle 2* (*Nabopolassar Chronicle*) comes to an end and royal inscriptions become undatable. Here is some information from the *Babylonian Chronicles*:

[The twenty-first year]: The king of Akkad stayed home (while) Nebuchadnezzar (II), his eldest son (and) the crown prince, mustered [the army of Akkad]. He took his army's lead and marched to Carchemish which is on the bank of the Euphrates. He crossed the river [*to encounter the army of Egypt*] which was encamped at Carchemish. [...] They did battle together. The army of Egypt retreated before him. [5] He inflicted a [defeat] upon them (and) finished them off completely... For twenty-one years Nabopolassar ruled Babylon. [10] On the eighth day of the month Ab he died. In the month Elul Nebuchadnezzar (II) returned to Babylon and on the first day of the month Elul he ascended the royal throne in Babylon.

In (his) accession year Nebuchadnezzar (II) returned to Hattu. (Grayson 1976: 99–100 = *Chronicle 5* Obverse: 1)

The fourth year: The king of Akkad mustered his army and marched to Hattu. [He marched about victoriously] in Hattu. In the month Kislev he took his army's lead and marched to Egypt. (When) the king of Egypt heard (the news) he *m[ustered]* his army. They fought one another in the battlefield and both sides suffered severe losses (lit. they inflicted a major defeat upon one another). The king of Akkad and his army turned and [went back] to Babylon. The fifth year: The king of Akkad stayed home (and) refitted his numerous horses and chariotry. The sixth year: In the month Kislev the king of Akkad mustered his army and marched to Hattu. He despatched his army from Hattu and [10] they went off to the desert. They plundered extensively the possessions, animals, and gods of the numerous Arabs. In the month Adar the king went home. The seventh year: In the month Kislev the king of Akkad mustered his army and marched to Hattu. He encamped against the city of Judah and on the second day of the month Adar he captured the city (and) seized (its) king. A king of his own choice he appointed in the city (and) taking the vast tribute he brought it into Babylon. (Grayson 1976: 101–2 = *Chronicle 5* Reverse: 5)

Berosus, *History of Chaldaea* 3 (*apud* Josephus, *Ant.* 10.11.1 §§220–26) also mentions the death of Nabopolassar and how Nebuchadnezzar marched across the desert to reach Babylon quickly and assert his claim to the throne.

The later Neo-Babylonian kings are discussed at §5.2.11.

5.1.4.3 Jehoiachin Documents

Tablets from Babylon are generally believed to mention Jehoiachin who was in captivity:

- (a) To Ya'u-kin, king [of the land of Yahudu].
- (b) ½ (PI) for Ya'u kinu, king of the land of Ya[hu-du]
 2½ *silā* for the fi[ve]sons of the king of the land of Yahudu
 4 *silā* for eight men, Judaeans [each] ½ [*silā*]
- (c) ½ (PI) for Ya'u [-kinu]
 2½ *silā* for the five sons
 ½ (PI) for Yakū-kinu, son of the king of the land of Yakudu
 2½ *silā* for the five sons of the king of Yakundu by the hand of Kanama.
- (d) Ya]'u-kinu, king of the land of Yahudu

[. . . . the five sons of the king] of the land of Yahudu by the hand of Kanama.
(Weidner 1939; *DOTT*: 84–6)

5.1.4.4 Texts from āl-Yāhūdu and Našar

A preliminary report has recently been made on an archive in private hands that was obtained on the antiquities market (Pearce 2006). The tablets range in date from Nebuchadnezzar (year 33, ca. 572 BCE) to Xerxes (year 13, ca. 473 BCE). Of the almost 100 tablets, a third each was composed at āl-Yāhūdu and Našar, with the final third composed at Babylon and other sites. The city āl-Yāhūdu had only recently come to light, in some texts from the Persian period (Joannès and Lemaire 1999). It means ‘city of Judah’, but the forms URU šá LÚ *ia-a-ḫu-du-a-a* ‘city of the Judahite’ and *ālu šá* ^{LU}*Yahūdāia* ‘city of the Jews’ also appear. These all suggest that the city was originally named after the first inhabitants, that is, deportees from Judah. The city seems to have been a neighbour of Našar and in the region of Borsippa and Babylon. Of the 600 individuals named in the text, about 120 have Yahwistic names. The texts are typical of those one might expect to find in an archive of this nature: receipts for sales of various sorts, promissory notes and notes of indebtedness, and leases of property or labour. Unfortunately, there is little information about the social status of the community members, since the profession of individuals in the texts is seldom recorded.

5.1.5 Egyptian Source: *Psammetichus Inscription*

In the fourth regnal year of Pharaoh Psamtek Neferibre they sent to the great temples of Upper and Lower Egypt, saying, ‘Pharaoh (Life, Prosperity, Health) is going to the Land of Palestine. Let the priests come with the bouquets of the gods of Egypt to take them to the Land of Palestine.’ And they sent to Teudjoy saying: ‘Let a priest come with the bouquet of Amun, in order to go to the Land of Palestine with Pharaoh.’ And the priests agreed and said to Pediese, the son of Essamtow, ‘you are the one who, it is agreed, ought to go to the Land of Palestine with Pharaoh. There is no one here in the town who is able to go to the Land of Palestine except you. Behold, you must do it, you, a scribe of the House of Life; there is nothing they can ask you and you not be able to answer it, for you are a priest of Amun. It is only the priests of the great gods of Egypt that are going to the Land of Palestine with Pharaoh.’ And they persuaded Pediese to go to the Land of Palestine with Pharaoh and he made his preparations. So Pediese, son of Essamtow, went to the Land of Palestine, and no one was with him save his

servant and an hour-priest of Isis named Osirmose. (Inscription of Psammetichus II = Griffith 1909: II, 95–6)

5.1.6 The Biblical Story: 2 Kings 21–25 and Parallels

NB: Only passages specifically mentioning the period are considered here. Other passages may well be from this time (e.g., the town lists of Joshua 15, 18 and 19) but are omitted here.

5.1.6.1 2 Kings//2 Chronicles (a dash marks omissions in the parallel writing)

21.1-18//33.1-20: Reign of Manasseh.

21.1//33.1: Manasseh begins to reign at age 12 and reigns 55 years.

21.2-9//33.2-9: He does what is evil by building altars to Baal and an Asherah, worshipping the host of heaven, making his son to pass through the fire, practising divination.

—//33.10-13: Manasseh taken captive to Babylon, where he repents and turns to Yhwh.

—//33.14-17: Manasseh builds Jerusalem's wall and removes the religious abominations.

21.10-15//—: Yhwh speaks against Manasseh through prophets, promising disaster on Jerusalem and to deliver the remnant of the people to their enemies.

21.16//—: Manasseh puts the innocent to death and fills Jerusalem with blood.

21.17//33.18: The other events of Manasseh's reign are found in the annals of the kings of Judah.

—//33.19: Words of Hozai recording Manasseh's sins and his prayer of repentance.

21.18//33.20: Manasseh dies and is buried.

21.19-26//33.21-25: Reign of Amon.

21.19//33.21: Begins reign at age 22 and reigns 2 years.

21.20-22//33.22-23: Wicked like his father.

21.23-26//33.24-25: Assassinated in a conspiracy.

22.1–23.30//34.1–35.27: Reign of Josiah.

22.1//34.1: Begins reign at age 8 and reigns 22 years.

22.2//34.2: Is righteous like David.

—//34.3-7: Begins purge of shrines and cults in his 12th year.

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- 22.3-7//34.8-13: Josiah orders the cleansing of the temple in his 18th year.
- 22.8-10//34.14-18: Finding of scroll and reading of it to Josiah.
- 22.11-20//34.19-28: Prophetess Huldah is consulted about the contents of the scroll.
- 23.1-3//34.29-32: Josiah makes a covenant with the people to keep the laws of the scroll.
- 23.4-7//—: Cleansing of the temple and the removal of the *kemarim* from the shrines.
- 23.8-9//—: Priests of the high places brought to Jerusalem.
- 23.10-14//—: Other cults and objects removed from the temple and city.
- 23.15-18//—: Bethel purified, including destruction of Jeroboam's altar.
- 23.19-20//—: Rest of Samaria purified of its cult places and priests.
- //34.33: Summary of Josiah's reign.
- 23.21-23//35.1-19: Passover kept.
- 23.24//—: Divination eliminated.
- 23.25-27//—: Prophecy of Judah's destruction postponed because of Josiah's righteousness.
- 23.28-30//35.20-27: Josiah's death at the hands of Necho.
- 23.30-34//36.1-4: Reign of Jehoahaz.
- 23.31//36.2: Begins reign at age of 23 and reigns 3 months.
- 23.32//—: Is wicked like his fathers.
- 23.33-34//36.3: Removed by Pharaoh Necho and taken to Egypt, and tribute imposed.
- 23.34-24.7//36.4-8: Reign of Jehoiakim.
- 23.35//—: Jehoiakim collects money for tribute.
- 23.36//36.5: Begins reign at age 25 and reigns 11 years.
- 23.37//36.5: Wicked like his fathers.
- 24.1-2//—: Vassal of Nebuchadnezzar 3 years and then rebels; raids against Judah.
- 24.3-4//—: Happens because Jehoiakim sheds innocent blood.
- //36.6-7: Nebuchadnezzar takes Jehoiakim captive to Babylon.
- 24.5-6//36.8: Death of Jehoiakim.
- 24.7//—: King of Babylon had taken all land to borders of Egypt.
- 24.8-16//36.8-10: Reign of Jehoiachin.
- 24.8//36.9: Begins to reign at age 18 and reigns 3 months.
- 24.9//36.9: Wicked like his father.
- 24.10-17//36.10: Nebuchadnezzar takes the city and deports Jehoiachin and family to Babylon.

24.17–25.21//36.10–21: Reign of Zekeiah.

24.18//36.11: Begins reign at age 21 and rules for 11 years.

24.19–20//36.12: Wicked like Jehoiakim.

25.1–21//36.13–21: Rebels and Jerusalem taken by Nebuchadnezzar; king and people exiled.

5.1.6.2 Jeremiah

22.18–19: Prophecy that Jehoiakim would have the ‘burial of an ass’.

25.1: 1st year of Nebuchadnezzar is the 4th of Jehoiakim.

32.1–2: Babylonian army besieging Jerusalem in the 10th year of Zedekiah, which is the 18th of Nebuchadnezzar.

34.7: Only Lachish and Azekah of the fortified cities holding out against besieging Babylonians.

37.11: The Chaldean army lifts the siege of Jerusalem because of the Egyptian army.

39.1: Nebuchadnezzar besieges Jerusalem in the 9th year of Zedekiah, the 10th month.

39.2: Jerusalem falls in the 11th year of Zedekiah, 9th day of 4th month.

39.3: Babylonian officers set up quarters.

39.4–7: Zedekiah taken prisoner.

39.8–10: Exile of those left in the city.

43.8–13: Prediction that Nebuchadnezzar would conquer Egypt.

44.30: Prediction that Pharaoh Hophrah would be delivered into the hands of his enemies (Nebuchadnezzar?).

46–47: Prophecy of Nebuchadnezzar’s destruction of Egypt.

46.2: Egyptian army of Necho defeated at Carchemish by the Babylonians in the 4th year of Jehoiakim.

52.1: Zedekiah aged 21 when he became king and reigned 11 years.

52.3–4: Jerusalem besieged in the 9th year, the 10th day of the 10th month, by Nebuchadnezzar.

52.5–7: City falls in the 11th year, the 9th day of the 4th month.

52.8–11: Zedekiah taken prisoner to Babylon.

52.12–14: Jerusalem razed by Nebuzaradan in Nebuchadnezzar’s 19th year, 10th day of the 5th month.

52.15–16: Those left in the city exiled.

52.28–30: Captivities in 7th, 18th and 23rd year of Nebuchadnezzar.

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5.1.6.3 Ezekiel

26–27: Prophecy against Tyre.

26.7: Nebuchadnezzar will come against Tyre and destroy it.

29–30: Prophecy against Egypt.

29.8-16: Egypt to be ruined and desolate 40 years, then to become the lowest of the kingdoms.

29.17-20: Having failed to take Tyre, Nebuchadnezzar will be given Egypt as a reward.

30.20-26: The king of Babylon will break the arms of the king of Egypt.

5.1.6.4 Ezra

4.1-4: The enemies of Judah and Benjamin – ‘the people of the land’ – were brought to the region by Esarhaddon.

4.9-10: Men of Erech, Babylon, Susa and others had been settled in Samaria and elsewhere in Eber-Nari by Ashurbanipal(?).

5.1.6.5 Daniel

1.1-2: Nebuchadnezzar besieges and takes Jerusalem in the 3rd year of Jehoiakim.

4.1-34: Nebuchadnezzar’s ‘madness’.

5.1-30: Belshazzar, king of Babylon.

5.1.6.6 Analysis of the Text

The account of the seventh century in 2 Kings is part of the DtrH (§3.1.6.2). Most of the text of 2 Chronicles and some parts of Jeremiah parallel the narrative of 2 Kings. The relationship of Jeremiah to 2 Kings is not immediately apparent, but it seems plain that 2 Chronicles is mainly derived from 2 Kings; however, here and there are significant deviations. The main ones are the following:

- Manasseh’s alleged captivity in Babylon and subsequent repentance is given in 2 Chron. 33.10-17 but completely absent from 2 Kings.
- 2 Chronicles (33.14), but not 2 Kings, states that Manasseh built the

outer wall around part of Jerusalem.

- According to 2 Chron. 34.3-7 Josiah began the purge of 'foreign' cults in his twelfth year.
- Whereas 2 Kings 22.21-23 mentions briefly Josiah's Passover, 2 Chron. 35.1-19 goes into great detail about how it was celebrated.
- According to 2 Chron. 35.20-27 Josiah was killed in a battle with Pharaoh Necho, but 2 Kings 23.28-30 is unspecific and does not at all imply a battle.
- According to 2 Chron. 36.6-7 Jehoiakim was taken captive to Babylon, along with the temple vessels. On the other hand, according to Jeremiah he was to have 'the burial of an ass', cast outside Jerusalem unburied (Jer. 22.18-19).
- In spite of Josiah's far-reaching reform programme described in 2 Kings and 2 Chronicles, Jeremiah seems strangely silent about it.

On the stories in Daniel, see §5.2.6; §5.2.11.

5.2 Analysis

The following section is in part organized around the names of Judaeen kings. The reason is that this particular study is asking specifically about the biblical data: what could we regard as reliable if we had only the Bible? Therefore, it makes sense to shape the question broadly around the Judaeen kings.

5.2.1 Hezekiah

The account of Hezekiah's reign begins with a religious and cultic reform (2 Kgs 18.3-6). This reform had been widely accepted in scholarship (e.g., Albertz 1994: I, 180-6), but it is now also widely questioned (e.g., Na'aman 1995a). The problem is that it looks very much like the reform later ascribed to Josiah. Did Josiah try to revive what failed under Hezekiah, or did the biblical writer borrow from Josiah's story to improve Hezekiah's piety by literary invention? K. A. Swanson (2002) has suggested that what lies behind the story is Hezekiah's shift from Egyptian religious symbolism, which had been widespread, to aniconism and the rosette. Since the rosette was an Assyrian symbol, at least some of the change can be ascribed to Hezekiah's acceptance of Assyrian subjugation. L. S. Fried (2002) argued that the archaeology of the *bāmôt* does not support the alleged cult reforms of the text. In a more

recent study, Na'aman (2002) looked at the abandonment of cult places as a result of alleged cult reforms, with special emphasis on Arad and Beersheba; he concluded that these represented an attempt to consolidate royal power. Z. Herzog, however, has argued that the cancelling of the temples at Arad and Beersheba supports the story of Hezekiah's reform (2001: 65–7).

The analysis of the biblical narrative has been an important part of the discussion relating to historicity. On the invasion of Sennacherib, more than a century ago B. Stade (1886) produced a basic analysis that has continued to dominate the literary discussion. He noted that 18.14–16 had already been recognized as an insertion from a good source that was early. He then argued that 18.13, 17 to 19.9a was parallel to 19.9b–37. Neither of these narratives could be considered trustworthy historical sources but were both 'legendary', even if here and there they contained a correct historical datum. B. Childs (1967) refined Stade's analysis, with the following terms:

Account A: 2 Kgs 18.13–16.

Account B₁: 2 Kgs 18.17–19.9a, 36–37//Isa. 36.1–37.9a

Account B₂: 2 Kgs 19.9b–35//Isa. 37.9b–36

Attempts to rationalize the various narratives in the biblical text already began in the infancy of Assyriology. In 1864 George Rawlinson first suggested that Sennacherib had invaded twice (1864: II, 30–46), the events of 2 Kings 18.17–19.37 relating to this second invasion. This conclusion was not made on the basis of the Assyrian inscriptions but the requirements of the biblical text, as he admitted (1864: II, 439 n. 4). Thus, the idea of reconciling the two sets of texts by postulating more than one invasion was already present not long after the Assyrian inscriptions had begun to give new understanding to the events of Sennacherib's reign. The question continued to be debated until the present, with eminent scholars for more than one invasion (J. Jeremias, R. W. Rogers, L. L. Honor, W. F. Albright, J. Bright) and others against it (J. Wellhausen, E. Schrader, E. Meyer, J. A. Montgomery, J. Meinhold, A. Alt, M. Noth, A. Parrot, A. T. Olmstead, J. Gray, H. H. Rowley) (see the survey in Grabbe [ed.] 2003: 20–34). It was W. H. Shea (1985) who advanced well beyond Bright with a lengthy circumstantial argument that Sennacherib had conducted a second invasion after 689 BCE; however, Shea's argument was refuted point by point by Frank Yurco (1991).

The Egyptian data were crucial to the question: when did Taharqa take the throne and what was his age in 701 BCE? K. A. Kitchen argued that the Egyptian inscriptions allowed only one invasion (1983; 1986: 154–61, 552–9). A. Rainey (1976), Yurco (1980: 222–3) and Kitchen (1986: 164–70) asserted that new grammatical studies and improved readings of the

text made it now clear that Taharqa was at least 20 years old in 701 BCE and fully capable of leading a military campaign as crown prince. However, it should be noted that not all Egyptologists agree on this interpretation. D. Redford (1992a: 351–3, esp. n. 163) still read the relevant inscription as saying that Taharqa was only 20 when he came from Nubia to Egypt in 690, while J. von Beckerath argued that Taharqa came to Egypt in 700 BCE at the earliest and probably later (1992: 7). Most recently, though, a new inscription has been assigned to the reign of Taharqa (Redford 1993), and Shea (1997; 1999) has argued that this inscription is to be connected to an invasion of Sennacherib, though not the one in 701 BCE but a postulated ‘second invasion’. M. Cogan, however, has dismissed this argument, pointing out that those deported by the Egyptians could not have been the Assyrian army and that subsequent Assyrian rule has no place for such a defeat of the Assyrian army (2001; on the chronology of the Twenty-Fifth Dynasty, see Redford 1999). We can sum up the situation by noting that as long as detailed information on Sennacherib’s reign after 689 BCE remains undiscovered, we cannot absolutely rule out a second Assyrian invasion of Judah; however, all the available evidence presently known is against this scenario.

So how reliable are the accounts in 2 Kings 18–20? Noth gave an interpretation that depended almost entirely on 2 Kings 18.16–19 (along with the Assyrian inscriptions); the rest of the biblical account was essentially ignored (1960: 266–9). This was essentially the consensus of the discussion by the European Seminar in Historical Methodology (Grabbe [ed.] 2003: 308–23): Account A was the most widely accepted as reliable but Accounts B₁ and B₂ were thought to be much later and unreliable compositions. N. Na’aman (2003) argued that B₁ was probably written in the mid-seventh century. B₂ was modelled on B₁ but was rather later, probably in the late Neo-Babylonian period. E. Ben Zvi (2003) put the final composition of 2 Kings 18–20 in the post-exilic period, the purpose being to contrast Hezekiah’s experience with Zedekiah’s and explain why Jerusalem was conquered by the Babylonians. Going against the consensus, C. R. Seitz (1993) doubted whether the Annals of Sennacherib can be said to confirm 2 Kings 18.14–16 and concluded that, whatever its basis in fact, Account A must still be interpreted in the larger redactional context of 2 Kings; it may even be intended to tone down the otherwise positive account of Hezekiah. K. Smelik (1992) argued that historical reconstruction must be based on 18.13–16 and Assyrian sources, which can be reconciled ‘in all essentials’. The narratives were written as a reaction to the destruction of the Jerusalem temple, retrojecting current problems into the past; thus, they cannot be used for historical reconstruction of 701 but rather of 586.

W. R. Gallagher's study (1999) 'most closely adheres' to the approach that uses all sources and assumes that they are largely reliable and comes to some quite conservative conclusions about it. He accepts that Account A is from a separate source but rejects the division of Account B into two separate sources and the idea that they form two accounts of the same events. The B account as a whole is by and large reliable. He would excise the name 'Tirhakah' from 19.9 but accept an Egyptian-Cushite force of some sort. The interpretation of 19.35 as referring to a plague is looked at sympathetically but seen as uncertain. His reconstruction of events is thus heavily influenced by the biblical Account B read as a sequential narrative (Account A is seen essentially as an overall summary). Yet C. Hardmeier (1989) also took up Childs' challenge. His solution was to dismiss any association of 2 Kings 18–19 with Sennacherib but to connect it with events shortly before the fall of Jerusalem. Instead of describing the siege of Jerusalem by Sennacherib, these chapters were a fictional creation to support those nationalists who wanted to resist the Babylonians. Written in 588 BCE, with the same background as described in Jeremiah 37–40 (but from the opposite perspective), they held up Sennacherib's invasion as an exemplar of how God would intervene to save his people. When this did not happen, the narratives were re-interpreted as an actual description of events in the time of Hezekiah. He thus rejects the consensus that the A narrative is an actual report of what happened. Hardmeier's is a major challenge to much previous thinking. Although others are willing to argue that parts of the Hezekiah narrative (particularly the B narrative) are even later than Hardmeier proposes (e.g., Na'aman 2003), his argument that the entire narrative is a fictive creation goes further than most are willing to go.

In an important study, S. Parpola showed that the name of Sennacherib's assassin had been wrongly interpreted for many years. It should be read as Arda-Mulīšši of which the biblical Adrammelech (2 Kgs 19.37) is a corrupt but recognizable form. A question is how to take the visit of Merodach-baladan to Jerusalem. Because a king of Babylon, Marduk-apla-dan actually existed, some have wanted to suggest that this is a plausible story: the Babylonian king, who was rebelling against the Assyrians, was seeking support and allies. This might seem to be a valid argument until one considers the distance that Judah lay from Babylon and the lack of any possibility of giving help. The story looks more like an explanation of why Jerusalem fell to the Babylonians.

One of Childs' main contributions to the debate was the comparison of the Rabshakeh's speech with that of an Assyrian document, leading Childs to argue that the B₂ biblical account reflected historical reality (1967: 78–93; Saggs 1955a; 1955b; 1956). This view seems to have gone unchallenged

until 1990 when an article by Ehud Ben Zvi undermined the whole basis of the argument by showing that the alleged parallels were made up of common biblical language, and the reference to Hezekiah's reform shows Deuteronomic features (1990). Following on this are studies by Klaas Smelik (1992), who considers the speeches free compositions by the author, and D. Rudman (2000), who points out the resemblance of the Rabshakeh's speech to biblical prophetic language. However, W. R. Gallagher (1999) has recently argued that both this passage and Isa. 10.5-19 were written close to the time of the alleged speech and are summaries of it.

The 'fourteenth year' (2 Kgs 18.13) has been a major difficulty since the Assyrian inscriptions showed that Sennacherib's invasion was in 701 BCE. According to 2 Kgs 18.1, Hezekiah became king in Hoshea's third year, while Samaria fell in Hezekiah's fourth year. If so, the events of 701 would have taken place about Hezekiah's twenty-fifth year, not the 'fourteenth'. Rawlinson simply emended the text to 'twenty-seventh', without giving any justification for this change except to try to reconcile the biblical and Assyrian accounts (1864: II, 434 n. 12). J. A. Montgomery (1951: 483) also emended, to the 'twenty-fourth year', but he expressed the view that the synchronism with Hoshea's reign was an error. Several other solutions have been advanced in the past few decades. One was by A. K. Jenkins (1976) who argued that the 'fourteenth year' refers to the invasion under Sargon II about 713–711 (Isaiah 20). According to this explanation the original account had an anonymous Assyrian king, but this king was later identified with Sennacherib. This interpretation depends on the 'high chronology' which makes Hezekiah's reign about 727–698 BCE. Very recently a similar explanation was given by J. Goldberg (1999) who argued for a 'limited invasion' of Palestine by Sargon II in 712 BCE, referred to in 18.13-16 and later confused with the 701 invasion. Hayim Tadmor and Michael Cogan put forward the case that the 'fourteenth year' was a reference to Hezekiah's illness, an incident that originally preceded the account of Sennacherib's invasion. Since Hezekiah's life was extended for 15 years after his illness, this gave the 29-year total of his reign.

In 1994 Na'aman surveyed the arguments on both sides and proposed that, though not conclusive, the balance of evidence favoured taking the fourteenth year as correct and dating Hezekiah's reign ca. 715–686 (1994a). Sennacherib's inscriptions continued to be revised by scribes at least until 691, yet Hezekiah's death is not mentioned, making it unlikely that he died in 698 as some have thought. The problem created with regard to Manasseh's reign by this dating is resolved by assuming a ten-year co-regency between Hezekiah and his son. B. Becking (1992; 2001; 2003) also addressed some of the questions of chronology. He has argued for the dating of the fall of

Samaria to 723 BCE, a year earlier than the conventional 722. He accepts that the synchronism made by the Deuteronomic editor between the reigns of Hoshea and Hezekiah (2 Kgs 18.9-10) is based on Judaeen archives. This means that the fourteenth year of Hezekiah has to be either spring 715 to spring 714 or autumn 716 to autumn 715. At that time Sargon II dispatched an expedition to Palestine that was relatively peaceful. This may have been led by the crown prince Sennacherib. This was the 'first campaign' of Sennacherib, dated to the summer of 715 BCE. Becking thus hypothesizes two campaigns of Sennacherib, but his schema differs from the conventional one in that the 701 invasion was his second campaign. (For further on Becking's chronology, see §4.2.4)

The current situation can be stated succinctly as follows. The firmest datum we have at the end of the eighth century is the invasion of Sennacherib. It can be precisely calculated to 701 BCE. We have detailed descriptions in the Assyrian sources, including mention of local Palestinian rulers by name (e.g., Hezekiah), and the widespread destruction left a distinct mark in the archaeological record. There is substantial agreement that reliable memory of this is found in 2 Kgs 18.13-16, and rather less reliable memory in various other parts of 2 Kings 18-20 (Grabbe [ed.] 2003). The 'two-invasion' hypothesis, although once widely accepted, looks now to be in tatters. The main extra-biblical support has collapsed. Although Sennacherib's reign is poorly documented after 689 BCE, there does not seem to be any room for another campaign to Palestine.

Although still debated, the weight of opinion seems to be that Taharqa was capable of leading a military expedition against the Assyrians in 701 BCE. Whether he did or not is naturally still a matter of debate, but the reason for his mention in 2 Kgs 19.9 probably derives from his later image as the great Egyptian (Nubian) king who stood up to Assyria (Dion 1988). However, the chronology of Hezekiah's reign remains disputed, and the way in which Jerusalem avoided a siege is considerably debated. How much longer Hezekiah ruled after 701 and what events took place in his reign are not matters of agreement. Our only information is from the biblical text, and even the sequence of events of the original story is thought to be disturbed, making it that much more difficult to ask questions about their basis in historical reality. For a continuation of the debate, see Grabbe ([ed.] 2003: esp. 308-23). As for Hezekiah's cult reform, there is still considerable question whether such took place, though the story might reflect some sorts of events during Hezekiah's reign.

5.2.2 Manasseh

What emerges from recent study is the importance of the reign of Manasseh. Far from being a time of depravity and fear, many think it represents a remarkable recovery from the devastations of Sennacherib. It must have given many Judaeans a return to some sort of prosperity and hope for the future. Of course, the name of Manasseh is one of the blackest in the biblical text. He is perhaps equalled – but not surpassed – only by Ahab and Jezebel. This suggests that the long reign ascribed to him is likely to be a firm part of the tradition and thus to have some basis in fact. Manasseh's existence is well attested in the Assyrian inscriptions. He is named as an apparently loyal subject paying the required tribute to both Esarhaddon and Ashurbanipal (though it has been pointed out that Manasseh's tribute is smaller than that of his neighbours [Finkelstein and Silberstein 2001: 265]). He also supplied military assistance for Ashurbanipal's attack on Egypt.

What the archaeology suggests is that Judah made a significant recovery from the disaster of 701 (Finkelstein 1994a; Finkelstein and Silberman 2001: 264–74). The important agricultural region of the Shephelah remained sparsely populated, probably the larger part of it having been removed from Judahite control. Elsewhere, though, settlements were re-established in destroyed southern areas, possibly with even a population increase. Settlements were also pushed into the marginal desert areas to make use of all possible land for agricultural purposes. Although dating is not easy, the suggestion is that this happened under Manasseh's leadership. Manasseh seems to have been responsible for building a city wall (2 Chron. 33.15), which could be the one dating from the seventh century discovered on the eastern slope of Jerusalem's southeastern hill (Tatum 2003: 300). It has also been proposed by E. A. Knauf that Manasseh built some prestige projects, including the Siloam tunnel (Knauf 2001b) and a palace at Ramat Raḥel (Knauf 2005: 170).

There is also some evidence of the part played by Judah in the economy of the Assyrian empire, a role that would have benefited the inhabitants of Judah. The territory of Judah formed a significant link in the caravan trade from Arabia, which the Assyrians would have controlled. The Idumaeon plateau gained a significant population at this time, with the trade route leading through the valley of Beersheba and the southern coastal plain to Gaza. There are indications of contact with South Arabia, and the seventh-century forts at Qadesh-Barnea and Haseva might have been built with the protection of this trade in mind. A major olive-oil production centre existed at Tel Miqne (usually identified with ancient Ekron); however, the olives were not grown locally but would most likely have been imported from the Samaritan and Judaeon highlands.

Of particular interest is the apparent increase in written material. The finds indicate a greater quantity of written objects preserved from the seventh century: seals/bullae, ostraca and inscribed weights (Finkelstein and Silberman 2001: 270, 280–1; Stern 2001: 169–200). This ‘explosion of writing’ has been explained as evidence of an increase in the bureaucracy (Finkelstein and Silberman 2001: 270; Stern 2001: 169) and even that Judah had become a fully developed state by this time (Finkelstein and Silberman 2001: 281, 284). These conclusions seem quite reasonable ones. The sorts of written objects catalogued here do look like the type of written material that would be the product of the bureaucracy and state administration. Whether they are evidence of greater general literacy, however, is another issue (§5.2.4; §3.2.5).

The question of an imposed Assyrian cult has been much debated in recent years. From the early days of cuneiform study it was argued that the Assyrians imposed their god Ashur on conquered peoples (interestingly, apparently first proposed by George Rawlinson [McKay 1973: 1–4]). This consensus was challenged by two works that appeared about the same time. First, J. McKay (1973) argued that there are no indications of Assyrian cults in any of the accounts of the kings under Assyrian rule: Ahaz, Hezekiah, Manasseh, Josiah. McKay also pointed out the importance of astral cults in the biblical account, which he ascribed to Canaanite practices rather than Assyrian. A telling point made by McKay is that the description of the cults set up under Manasseh and removed by Josiah indicates they were Syro-Phoenician, not Assyrian. Independently of McKay, M. Cogan (1974) also argued against an imposed Assyrian cult, his focus being on the cuneiform texts to try to determine Assyrian practice. He noted that the Assyrians made good use of the concept of divine abandonment by the gods. Although the Assyrians occasionally destroyed images and temples of recalcitrant peoples, their normal practice was to take the images of native gods to Assyrian territories. The treaties with conquered peoples invoked Ashur, and the Assyrians certainly understood submission to Assyria as submission to Ashur, yet this does not imply cultic obligations. Regions which were turned into Assyrian provinces, however, were considered Assyrian and their peoples Assyrian citizens; in such cases an Assyrian cult was established (before ‘Ashur’s weapon’, the symbol of Assyrian rule) though native cults were not prohibited. Then Hermann Spieckermann (1982) replied to both monographs, though most of his arguments related more to Cogan’s work. Cogan (1993) eventually replied to Spieckermann and, not surprisingly, reaffirmed the view that there was no evidence for imposed cults and that those described in the Bible seem to be indigenous ones. This is unlikely to be the last word on the subject, but although Manasseh seems to have been

a loyal vassal, the cults in existence under his rule were more likely to be old indigenous ones rather than those imposed from the outside. Finkelstein and Silberman (2001: 265) think that there was a return to popular religion by the people of the countryside after Hezekiah's failed cultic reform (assuming there was such a reform). Others would argue that nothing innovative from a religious point of view happened under Manasseh; rather, he was just blamed for what had been the traditional religion among the people for many centuries. This does not, though, rule out foreign influence, such as from Assyro-Aramaic astral cults (§5.2.4).

One of the most curious episodes related in the Bible is about Manasseh's deportation to and imprisonment in Babylon, followed by a return to Jerusalem and his throne and by repentance for his wicked deeds. This story is found in only one passage, significantly in Chronicles (2 Chron. 33.10-17), but not a hint is found in 2 Kgs 21.1-18. Furthermore, other biblical passages know nothing of Manasseh's repentance: Jer. 15.4 speaks of Manasseh's wickedness; 2 Kgs 23.12 says that Josiah – not Manasseh – removed the abominations; and even 2 Chron. 33.22 says that Amon was wicked like Manasseh. If Manasseh actually did all this, why would the Deuteronomist omit it? He was either ignorant of the information or he deliberately suppressed it. It is difficult to believe that if such an incident took place he had no information, so we must assume that he knew of Manasseh's repentance but purposefully ignored it. Is this likely? Yes, perhaps if the idea of an act of repentance on Manasseh's part created problems with his underlying pattern of presentation, but this would be a serious reflection on the claim made by some that the Deuteronomist was writing history.

Yet there is an interesting parallel which suggests that what is alleged to have happened to Manasseh was not unlikely in and of itself. During Ashurbanipal's invasion of Egypt, some of his allies plotted to rebel. When he discovered the plot, he removed them all from office except Necho II who was allowed to remain on the throne (*ANET* 294–5). It is possible that Manasseh was also plotting to rebel (Elat 1975b: 67). If so, he might have been allowed to regain his throne after appropriate punishment, just as Necho was. However, it must be admitted that this is entirely speculation – there is no evidence that Manasseh plotted to rebel or that he was punished by either of the Assyrian kings under whom he was vassal.

As has been pointed out, having Manasseh repent would fit the Chronicler's purpose very well: because Manasseh lived so long, he could have done so only if he repented of his evil deeds. To the Chronicler's way of thinking, length of life was a reward for obedience. Yet is it likely that the Chronicler invented the story out of whole cloth? It is always possible, but it seems more likely that he had knowledge – however tenuous – of some incident

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involving Manasseh, which is indeed indicated by his citation of the ‘words of Hozai’. The events under Ashurbanipal, in which Manasseh and others had to accompany the Assyrian king in his invasion of Egypt, might well be such a pretext. In sum, though, the story of Manasseh’s having been arrested and taken ‘with hooks and bound in bronze fetters’ to Babylon is unlikely.

5.2.3 *Amon*

We have no data on him other than what is in the Bible. He is unlikely to have been invented. It was unusual for a king to be assassinated, and there is nothing about Amon to give this a literary significance. Thus, it is likely to have happened. However, the figures given for his age look suspect: although it is theoretically possible that he had a child at age 16, this seems highly improbable. Possibly the problem is with the age of Josiah (see next section), though in cases of the sudden death of a king a minor child might well take the throne.

5.2.4 *Josiah*

Despite the importance given to him in the biblical tradition, Josiah is known only from the biblical text. Neither the surviving Babylonian nor Egyptian records contain any reference to him. We are left with archaeology and the biblical text with which to make sense of his reign, though the Egyptian material and the Babylonian chronicles provide useful background and contextual information. Many past reconstructions have depended on the picture in 2 Chronicles, even in those aspects which differ at significant points from those in 2 Kings.

One theory that has held considerable sway for a number of decades is that Josiah was attempting to create a ‘greater Israel’, perhaps on the model of the Davidic kingdom. There are many obvious parallels between Josiah and David, though one could put these down to literary creation rather than actual activity of the ruler. The ‘righteousness’ of both kings is the most obvious contact, but the conquest of territory is another that many scholars have managed to glean from the biblical material: the attempt to return to a ‘greater Israel’ and a recovery of former glory. It has been argued that, although Josiah’s reform was indeed religious, the basis of it was economic (Claburn 1973).

Na’aman (1991: 33–41; 2005: 210–17) has argued, however, that there was no political vacuum which gave Josiah room to try to found a new

Davidic 'empire'. Rather, the declining Assyrian power in the west was matched by the growing power of Egypt; indeed, there may have been an orderly transfer of territorial control by mutual agreement (Na'aman 1991: 40). Miller and Hayes (1986: 383–90) had already argued that Josiah was an Egyptian vassal his entire reign. Na'aman has gone on to create a picture of Judah as a vassal state during the entirety of Josiah's reign, first under the Assyrians and then under the Egyptians. This gave only very limited scope for expansion of territory. There is some evidence of shifting the border as far north as Bethel. However, the expansion further north into the Galilee or west into the area of Philistia is unjustified from either archaeology or the text.

A further potential source of information about Judah's boundaries are the town lists of Joshua (15.21–62; 18.21–28; 19.2–8, 40–46). It was argued by A. Alt that the lists of the southern tribes actually reflected Josiah's kingdom, and the question has now been investigated at length by Na'aman (1991; 2005). It is impossible to summarize the detailed textual analysis here, but the lists of Judah and Benjamin are the main ones in question. The northern border should be set along the Bethel–Ophrah line, which was north of the traditional border of the kingdom of Judah (Josh. 18.21–28); Jericho is also included, though it had previously been an Israelite town. If Josh. 21.45–47 is deleted as an addition by the editor, the list includes the eastern Shephelah. 2 Kings 23.8 makes reference to the territory 'from Geba to Beersheba', which is likely to be an indication of Judah's actual extent under Josiah. Therefore, the reference to Qadesh-Barnea (if this is indeed the site indicated in Josh. 15.23) is probably to be seen as an addition to the list, but most of the sites are no further south than Beersheba. The main point made by Na'aman is the extent to which archaeology (e.g., the rosette seal impressions) and other sources of data fit with these lists in Joshua.

The ostraca from Mešad Hashavyahu do not seem to show a Judean outpost, as often alleged, but probably an Egyptian one with some Judean soldiers (Fantalkin 2001: 139–47; Na'aman 1991: 44–6; 2005: 220–2). Other names in the texts are Phoenician, for example. Neither does the text of Ostrakon 1 provide evidence of Josiah's religious reform. The sender of the ostrakon appeals to the humanity of the recipient, not to the law of the king or the Torah. Similarly, the Arad ostraca indicate a contingent of Greek mercenaries in that area, which was probably in the employ of the Egyptians.

With regard to borders, R. Kletter (1999: 40–3) considers the question in the light of several types of artifacts. He argues that archaeology cannot pinpoint accurate borders nor establish the political affiliation of single sites. The overwhelming majority of the artifacts considered by him fit

more or less with the Judaeen heartland and do not indicate large-scale expansion. Various explanations have been given for those finds of artifacts outside the heartland; however, only the western Shephelah shows much of a concentration. Although Judaeen artifacts have been found at Qadesh-Barnea and Mešad Ḥashavyahu (Judaeen inscribed weights, pottery vessels, Hebrew ostraca), the material culture of both sites is mixed, as is that of the area bounded by Ekron–Gezer–Tel Batash; perhaps a similar phenomenon occurred in the Negev. According to Kletter, they could show a mixed population or a temporary Judaeen domination; in any case, they cannot help in defining Judah's borders because they are isolated sites outside any sequence of Judaeen settlements. They thus cannot prove or refute the possibility that Josiah expanded his territory for a short duration, though the few finds outside Judah are best explained by trade or exchange.

The significant increase in written artifacts (seals, seal impressions, ostraca, inscribed weights) has been interpreted as demonstrating a greater degree of literacy in Judah under Josiah. This seem to me to be doubtful; see further the discussion under §3.2.5.

It was once conventional to accept Josiah's reform at face value, but the question is currently much debated (Alberty 1994: 198–201; 2005; Lohfink 1995; Davies 2005; Knauf 2005). We have no direct evidence outside the biblical text, which makes us at least ask whether it is an invention of the Deuteronomist. The alleged absence of any reference to this reform in Jeremiah has always been a major puzzle. Some have found allusions here and there, but one has to admit that they are surprisingly obscure. Considering Jeremiah's overall message and position, he should have embraced such a reform and made copious comments about it. Some have seen evidence in the material remains (e.g., Uehlinger 1995; 2005), but others have argued against it (e.g., Niehr 1995). The central passage is 2 Kings 22–23, however. It is widely agreed that this passage has been the subject of Deuteronomistic editing, leaving the question of how much might be Deuteronomistic invention. C. Hardmeier (2005) and C. Uehlinger (2005) argue that at the heart of 2 Kings 22–23 is a simple list of reform measures affecting mainly Jerusalem and perhaps Bethel, to which the Deuteronomistic editors have added an extensive superstructure that makes the reform much more extensive in scope and geography than the original list. Uehlinger argues that the original list – but not the much-expanded present text – is supported by the archaeology and iconography. Knauf, however, argues against any 'core' from the time of Josiah (2005: 166–8).

A related issue is the statement in 2 Chron. 34.3–7 that Josiah began to purge the country of the various shrines and cults in his twelfth year (i.e., at age 20). This does not accord with 2 Kgs 22.3–7 which has the reform follow

the discovery of the law book in the temple. This is not an easy issue to address because the description in 2 Kgs 22–23 is an idealized one: the only question is how idealized. This is perhaps why a significant opinion of scholarship has accepted the statement in 2 Chron. 34.3–7 that Josiah began his reform six years before the finding of the law book. Yet as Na'aman (1991: 38), among others, has pointed out, the theological motives of the writer of Chronicles have had their way here as elsewhere. He notes that when Josiah reached the age of majority at 20, it would have been 'unthinkable' in the theological world of the Chronicler that he would have done nothing about the 'pagan' shrines for another six years. Thus, it was theologically desirable that Josiah begin his reform in his twelfth year rather than wait until his eighteenth. Na'aman has also connected the dating of the reform (which he puts in 622 BCE) with the height of the crisis in Assyria during the revolt of Babylon in 626–623 BCE. It may be that the Assyrian ruler Sin-shar-ishkun's problems were sufficient to give Josiah confidence to initiate his reforms without being in danger of attracting Assyrian disapproval, while the Egyptians who replaced them may not have been particularly concerned.

Many have followed the narrative of 2 Chronicles which has Josiah die in a pitched battle. This seems unlikely, however, since Judah as an Egyptian vassal state is not likely to have been in a military position to challenge the Egyptian army. On the other hand, a vassal king would have been expected to appear before the new ruler (Necho II [610–595 BCE] in this case) to pay homage and swear allegiance. Doubts have been expressed about the fate of Josiah for some considerable period of time (Na'aman 1991: 51–5, with earlier literature). 2 Kings is clearly reticent to tell what happened, but Na'aman's argument that Necho had Josiah executed for suspected disloyalty of some sort makes the most sense, not only from the general historical situation but also from a close reading of the text in 2 Kings.

5.2.5 Jehoahaz

Nothing is known of him, though he has only the brief reign of three months. What does fit is that he would have been removed from the throne by the Egyptians who were probably in control of the region at this time (as discussed under §5.2.4 above).

5.2.6 Jehoiakim

Jehoiakim is known only from the biblical text, yet his reign illustrates the external politics of the ancient Near East at this time and fits in well with them. Judah was clearly an Egyptian vassal, since it was the Egyptians who put Jehoiakim on the throne. But in Jehoiakim's fourth year Nebuchadnezzar gained control of the region after the battle of Carchemish, and Judah became the vassal of the Babylonians. He then rebelled after three years. Why? The answer is that in 601 BCE Nebuchadnezzar fought a costly battle with Necho II which inflicted considerable damage on both armies; indeed, it took the Babylonians several years to recover, as indicated by *Babylonian Chronicle* 5. It was after this battle that Jehoiakim rebelled. It was not until two years later that Nebuchadnezzar retaliated by fostering raids against Judah, and it was not until late in 598 that he sent an army against Jerusalem. 2 Chronicles 36.6 states that Nebuchadnezzar besieged Jerusalem and took Jehoiakim captive to Babylon, while Jer. 22.18-19 predicts that he would have the 'burial of an ass' (i.e., his carcass would be dragged outside Jerusalem and left exposed and unburied). Neither appears to be what happened; from 2 Kings 24 it looks as if Jehoiakim died a natural death only a couple of months or so before Nebuchadnezzar set siege to Jerusalem, and it was his son who paid the price for his rebellion. As for Dan. 1.1-2, it is completely confused, most likely based on a misreading of the narrative in 2 Kings and 2 Chronicles (Grabbe 1987: 138-40).

5.2.7 Jehoiachin

Although he reigned only very briefly, Jehoiachin is well attested. In the biblical writings his name is mentioned not only in 2 Kings and 2 Chronicles but also in Jeremiah (22.24, 28; 27.20; 28.4; 29.2; 37.1; 52.31), Ezekiel (1.2) and Esther (2.6). Jehoiachin is known (though not by name) from the *Babylonian Chronicles* which tell of Nebuchadnezzar's taking of Jerusalem and his carrying of the Judean king into captivity. Jehoiachin's name has also been preserved in the Jehoiachin tablets from Babylon. Thus, this young ephemeral ruler is better known from extra-biblical sources than the famous Josiah.

5.2.8 Zedekiah

The last king of Judah is known from the *Babylonian Chronicles* as the king placed on the throne by Nebuchadnezzar after his conquest of Jerusalem

in early 597 BCE. Zedekiah's name is known only from the biblical text, however. We have no Mesopotamian historical sources after 594 when the *Babylonian Chronicles* come to an end. Yet the inscription of Psammetichus II (595–589 BCE) describing a tour of Palestine fits a situation in which the king of Judah was constantly looking for ways to free himself from the overlordship of Nebuchadnezzar (§5.1.5). The book of Jeremiah describes a number of episodes involving the king or courtiers (see below). The rebellion and final siege and capture of Jerusalem are, unfortunately, not known from any Mesopotamian source. Yet in view of the detailed information confirmed for 2 Kings in the period before this, the reasonableness (for the most part) of the picture in 2 Kings, and the general background situation in the ancient Near East, it does not take much of a leap of faith to accept the general picture and the approximate date for the destruction of Jerusalem.

5.2.9 The Case of Jeremiah

In spite of R. Carroll (1986) and the problems of reading the text of Jeremiah as a contemporary, dispassionate biography, there are a number of statements in Jeremiah about external events that could be based only on contemporary knowledge (Grabbe 2006a). These involve the activities of Nebuchadnezzar and their dating. The writer knows about the battle of Carchemish and its correct dating (Jer. 26.2). He is aware of the siege of Lachish and Azekah (Jer. 34.6-7), a situation which has appeared in a remarkable way in Lachish Letter #4. Only some of the statements about Nebuchadnezzar can be checked with Mesopotamian sources, but the exact recital of events in particular years of the king have the look of authenticity.

In addition, Jeremiah describes the prophet's interaction with the Judaeans kings (especially Zedekiah) and various members of the court and temple. A whole network of supporters, opponents and relatives appears in the book. We are unlikely to find confirmation of this information outside the Bible. However, data from other texts – which do not seem to be the author's sources – may give some independent support to the reliability of this information for the most part (cf. Long 1982). Yet the find of a seal of 'Baruch son of Neriah the scribe' does not prove that Jeremiah had a scribe named Baruch (cf. Carroll 1997: 96–100), even if the seal is authentic. An authentic seal could help to confirm the existence of Baruch the scribe, but the question would still remain as to whether a high government official – as this Baruch would seem to be – would have had the interest and leisure to serve as Jeremiah's private scribe. As for the prophecies about Jehoiakim's having the 'burial of an ass' and Nebuchadnezzar's conquest

of Egypt, these seem to be mistaken. Perhaps they were genuine prophecies – that failed!

The book of Jeremiah contains a variety of material; I think most would admit to that even if they did not subscribe to the specific analysis of S. Mowinckel (1914) or others. Carroll's sharp observations have called into question the amount of personal material from Jeremiah himself (Carroll 1981), yet he did not explain adequately some of the material in the book. It seems likely that some of the data there can be explained best as coming from a contemporary writer. Unlike 2 Kings, however, the information is not likely to come from a chronicle. Although it refers to international events on occasion, a lot of it is highly individual to Jeremiah or those around him. The best explanation seems to be that a contemporary of Jeremiah's did write some sort of 'biography' of the prophet. If this suggestion does not commend itself, any alternative theory has to take account of how the book has some statements that match what we know of the wider history of the ancient Near East at the time.

5.2.10 *The Case of Nebuchadnezzar*

One of the most interesting points that has arisen out of this study is the number of statements about Nebuchadnezzar in 2 Kings and Jeremiah that accord with the contemporary sources. The rules of Jehoiakim, Jehoiachin and Zedekiah are tied closely to Nebuchadnezzar's reign. The battle of Carchemish is known and given the correct chronological position. Although Jehoiakim's rebellion is not explained in the biblical text, both the reason for it and the timing make sense in the light of Nebuchadnezzar's stand-off with Egypt in 601 BCE. The biblical account of the Babylonian king's taking of Jerusalem in 597 BCE fits with everything we know from Mesopotamian sources. The Jehoiachin tablets (§5.1.4.3) confirm the presence of Jehoiachin in Babylon, along with his sons and other Judahites. Not every reference to contemporary history is accurate (e.g., whether Nebuchadnezzar ever conquered Egypt), but the amount of specific information is remarkable.

When it comes to Ezekiel, however, the information is much more doubtful. It may be that Nebuchadnezzar besieged Tyre, since Ezekiel's original prophecy is 'corrected' later on. But there is no evidence for a 40-year period of desolation for Egypt, as predicted. 2 Chronicles seems to be dependent on 2 Kings, and none of the additional passages is proved to have reliable information, with the possible exception of the city wall. The one specifically relating to Nebuchadnezzar is Jehoiakim's Babylonian captivity, and this goes contrary not only to 2 Kings but also the *Babylonian*

Chronicles. As for Daniel, the writer seems to know of Nebuchadnezzar only through the biblical text. The siege of Jerusalem in Jehoiakim's third year is based on a partial misunderstanding of 2 Chronicles. The other stories in Daniel about Nebuchadnezzar seem at least in part based on legends rising out of the reign of Nabonidus. This was first recognized by W. von Soden (1935) but confirmed in a startling way by the discovery of 4Q Prayer of Nabonidus (see further the discussion in Grabbe 1987).

5.2.11 The 'Exile'

The 'exile' has been seen as a seminal period in Israel's history, though interpreted in widely different ways (Grabbe [ed.] 1998; Scott [ed.] 1997). It now seems to be agreed that a good deal of literary production went on during this time. The Deuteronomic History may have been compiled during this time or, according to some, had a second edition issued in that period (§3.1.6.2). What one thinks occurred depends a good deal on how one responds to the 'myth of the empty land' (Carroll 1992; Barstad 1996). Some have objected to this designation and insisted that the biblical text is correct: the land was basically empty (Oded 2003). Yet although O. Lipschits (e.g., 2005: 187 n. 11; 190 nn. 22–3) is sometimes critical of Barstad, he takes a definite stand against the 'myth of the empty land' (2004). Jerusalem itself was uninhabited (§5.1.1), but there was a thriving settlement in the Benjamin area and also settlements here and there in the area of Judah.

Most of our knowledge of Judah is based on archaeology, but we have knowledge of the Neo-Babylonian kings. Most of their actions do not appear to have had a direct effect on the people of Judah. One biblical tradition, however, has to be considered. This is the story of Belshazzar in Daniel 5. We know a lot about the reign of Nabonidus, the last king of the Neo-Babylonian empire, and his son Belshazzar (Beaulieu 1989). Although acting for his father while the latter was in Tema, Belshazzar was nevertheless never declared king. In any case, Nabonidus had returned to the city by the time of the Persian siege. Belshazzar is not mentioned in the account of the fall of Babylon (though Nabonidus is) and may not even have been alive at that point. Daniel 5 is an exciting drama but no more than that.

For Judah during the Persian period, see Grabbe 2004a.

5.3 Synthesis

Because of the nature of the sources and the study of history in the period of this chapter, a synthesis was already given above under §5.2 and will not be repeated here. Thus, all that needs to be done at this point is to indicate how the biblical text matches with the critical history of the period.

5.3.1 *Biblical Data Confirmed*

- In broad terms, Hezekiah's existence and revolt and Sennacherib's destructive invasion of Judah are supported by the Assyrian records and by archaeology, especially relative to the city of Lachish.
- Manasseh's existence and name are attested by the inscriptions of both Esarhaddon and Ashurbanipal.
- The one datum about Manasseh's reign found in Chronicles, but not in Kings, that is likely to be correct concerns his building of a city wall (2 Chron. 33.14): something very like this was found down the eastern slope of the southeastern hill by K. Kenyon.
- Although some of the data from seals are problematic, because of either problems of interpretation or questions of authenticity, some are supportive of the biblical data, to a lesser or greater extent. Perhaps one of the most likely is the one reading 'Gemaryahu son of Shaphan' (cf. Jer. 36.10, 12); another is the reference to an official of 'Baalisha', king of Ammon (cf. Jer. 40.14). Moderately high in probability is 'Azariah son of Hilkiah' (cf. 2 Kings 22–23; 1 Chron. 6.13; 9.11; Ezra 7.1). Of more moderate probability is the one reading 'Jaazaniah the servant of the king' (cf. 2 Kgs 25.23).
- Josiah's religious reforms – the aspect of his reign, and of the seventh century, of most interest to many modern scholars – are difficult to establish in any direct way. (There are some cogent arguments from textual analysis that a modest list of cult measures lies at the heart of 2 Kings 22–23 [Uehlinger 2005], but my concern here is not primarily with inner-biblical analysis.) But the disappearance of Yhwh's consort and astral symbols from the iconography suggest a significant religious change. This does not by itself establish Josiah's reforms but combines with other considerations to make the general biblical account (not necessarily the details) plausible.
- Pharaoh Necho (II) and his support of the Assyrians against the Babylonians are confirmed by Egyptian sources and especially the *Babylonian Chronicles*.

- Jeremiah's references to the battle of Carchemish and other activities of Nebuchadnezzar (II) are remarkably accurate, suggesting they would have been based on contemporary or near contemporary information.
- Jehoiakim's rebellion in Nebuchadnezzar's fourth year fits the events described in the *Babylonian Chronicles*, even though Judah is not specifically mentioned in the entry for that year.
- Nebuchadnezzar's attack on Jerusalem of 597, and the deposition of one king and the replacement with another, is given substance by the *Babylonian Chronicles*. Although the name of Jehoiachin is not found in the *Babylonian Chronicles*, it seems confirmed by the reference in the Jehoiachin tablets.
- One of the Lachish ostraca mentions the siege of Lachish and Azekah (cf. Jer. 34.6-7).

5.3.2 Biblical Data not Confirmed, though They may be Correct

- Amon's assassination is not likely to have been made up, especially since he seems to have made so little impact in the history of Judah.
- Surprisingly, nothing of Josiah's reign can be directly confirmed from extra-biblical sources, not only his cultic reform but even his existence, since he is not mentioned in any Assyrian or Egyptian sources so far discovered.
- The statement of 2 Chron. 34.3-7 that Josiah began his religious purge already in his twelfth year may be true, and many scholars have accepted it. However, it seems unlikely; otherwise, why was there such a reaction to finding the law book in the temple? (This consideration is apart from the arguments of those who maintain that the cultic reform was not a part of Josiah's reign.)
- Ezra 4.2 refers to a deportation to Palestine under Esarhaddon, but the exact place is not specified. Ezra 4.9-10 mentions various peoples brought to Samaria by 'Osnappar', a name unknown in Assyrian history, though some think it is a corruption of Ashurbanipal or possibly Esarhaddon. In addition to the problems with the two passages just noted, no such deportation is known under either Esarhaddon or Ashurbanipal. One can only say that it is possible in the present state of our knowledge.
- The names and actions of the various palace and temple officials with whom Jeremiah interacts may be correctly recorded. As noted above, there are internal data that suggest the plausibility of some of them. There are no external data except possibly some seal inscriptions in a

few cases.

- The destruction of Jerusalem by Nebuchadnezzar in 587/586 is likely to be correct, given the general accuracy of such data in this section of 2 Kings.
- Nebuchadnezzar may have besieged Tyre. The fact that Ezekiel first says he would conquer it and then admits that he did not is an indication of prophecies contemporary with the events.

5.3.3 Biblical Picture is Most Likely Incorrect

- 2 Kings has Judah throw off the Assyrian yoke under Hezekiah, and no hint of an imposition until after the death of Josiah. This is an omission, but it goes further: it seems to be a deliberate attempt by the compiler to mask the fact that Josiah was a vassal, probably first of the Assyrians and then of the Egyptians.
- 2 Chron. 33.10-17 has Manasseh being taken captive to Babylon. Although some event might lie behind this (§5.2.2), it is contradicted by the silence in 2 Kings, the general image of Manasseh in the Assyrian inscriptions (such as the lack of any indication of rebellion), and the unlikelihood that he would have been taken to Babylon if he had been taken captive.
- Although it is not impossible, Amon is unlikely to have had a child at age 16.
- The picture in 2 Chron. 35.20-27 that Josiah fought a pitched battle with Pharaoh Necho and was mortally wounded by an arrow is contradicted by 2 Kings 23.28-30 and looks like a literary topos.
- The 2 Chron. 36.6-7 statement that Jehoiakim was taken captive to Babylon fits neither the picture in 2 Kings nor that in the *Babylonian Chronicles*.
- The statement in Jer. 22.18-19 that Jehoiakim would have ‘the burial of an ass’ by being thrown out without a proper burial is contradicted by the statements in 2 Kings 24.6 that Jehoiakim simply ‘slept with his fathers’ – which usually means a peaceful death and burial.
- The prophecies of Jeremiah and Ezekiel that Nebuchadnezzar would conquer Egypt and that it would remain desolate for 40 years are contradicted by what we know of both Nebuchadnezzar’s reign and of Egyptian history.
- Daniel seems to have known little or nothing about this period except what the author read in the biblical text or had received in the form of very legendary material. Jerusalem was not besieged by Nebuchadnezzar

in Jehoiakim's third year. Nebuchadnezzar was not mad (or whatever word one wishes to use) for seven years. Belshazzar was not king of Babylon when the Persians captured it.

5.3.4 Biblical Picture Omits/has Gaps

- The truly devastating effects of Hezekiah's rebellion on the cities and villages of Judah are only hinted at; the text is at pains to establish Hezekiah's faithfulness to Yhwh in his rebellion against Assyria rather than the suffering he caused to his country and people. Also, as noted above, Judah remained an Assyrian vassal throughout the reigns of Hezekiah, Manasseh and Amon, and the first part of Josiah's reign.
- The real achievements of Manasseh's reign from a political, economic and social point of view (as attested especially in the archaeology) are completely ignored.
- The text is coy about giving the real reason for Josiah's death. The thesis that he was executed by Necho is plausible, though not certain, but it is unlikely he was killed in battle.

Part III

Conclusions

CHAPTER 6

‘The End of the Matter’: What Can We Say about Israelite and Judahite History?

We now return to consider our original aim: we set out to investigate what we can know about the history of Israel and Judah, keeping in mind questions of sources, method and current debates. This has been *prolegomena* to a history, not the history itself. It has attempted to address those issues and questions that have to be considered and answered before any history can be written. Just to remind ourselves, here is a brief summary of the principles of historical method used throughout this book (§1.3.3):

- All potential sources should be considered.
- Preference should be given to original sources.
- The *longue durée* needs always to be kept in mind.
- Each episode or event has to be judged on its own merits.
- All reconstructions are provisional.
- All reconstructions have to be argued for.

Among the sources – and questions – considered in each chapter has been the biblical text: to what extent is the Bible reliable – to what extent can we use it in writing our history? Confirmed minimalists and hardened maximalists already know the answer, it seems (though they generally claim to be open to the facts), but most scholars do not pitch their tents at these extreme ends of the spectrum. For those who do not immediately embrace or reject the text, no easy answer arises from our pages: we cannot say that the biblical text is reliable or unreliable, because it all depends on which episode or text one has in mind.

Some cannot understand why the biblical text seems to have been so ‘vilified’, as they would see it, but the answer is simple: the fundamental

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historical principle of distinguishing between primary and secondary (tertiary, etc.) sources. Primary sources are those contemporary (or nearly so) with the events they describe and usually have some other direct connection (eyewitness report, compilation from eyewitness reports or other good sources, proximity to the events or those involved in the events). Secondary sources are those further removed in time and space from the original events. The Bible is not being attacked or vilified, but it is, unfortunately, almost always a secondary source because of the long history of writing, compilation and editing. The complicated history of the biblical text has been partially worked out in the past two centuries, but there is still much unknown and much on which there is disagreement. Primary sources are not always trustworthy, and secondary sources may sometimes contain reliable information, and no two sources agree entirely. Thus, the historian has to make a critical investigation of all data, whatever the source. Yet the basic principle has to be maintained: primary sources normally take precedent, and secondary sources normally need some sort of confirmation.

We began with the *longue durée* (§1.2.2). The history of Palestine is heavily shaped by the landscape and climate. The land is divided between the fertile, more congenial north; the drier, less productive south; the desert margins in the south and east. There is great variation between the northern valleys, the central hill country, the Shephelah, the coastal plain, and the Negev. One cannot write a history in a vacuum, nor should one advance scenarios that depend on improbable population and material resources. The topography and similar factors resulted in many of the same urban sites being inhabited throughout the second and first millennium BCE. Jerusalem was important not because of great wealth or rich resources of Judah but because the rugged terrain gave it natural defences and made it generally not worth the effort to attack. There was also a cycle of alternation between agrarian settlement and pastoralism over the centuries, going up and down according to climatic, economic and other factors. Finally, Palestine lay between great powers to the south and north whose expansion and conflicts affected the situation of the whole Syro-Palestinian region.

The history of Israel begins in the second millennium BCE. We do not know when the name 'Israel' or an entity called Israel came into existence, but we have the first reference to both these just before 1200 BCE. Yet texts, as well as archaeology, take us further back into the second millennium BCE. Texts from Egypt mention a number of sites, well-known from the first millennium BCE, that already existed centuries earlier, including Jerusalem, Shechem, Megiddo, Akko, Lachish, Gaza, Ashkelon and Laish. We find, for example, that the cities (city-states) of Shechem and Jerusalem seem to have dominated the Palestinian highlands in the fourteenth century,

according to the Amarna letters. This does not mean that Jerusalem was a major urban area: it could have been little more than a citadel or country manor (insufficient archaeological remains have been found to tell us), but it certainly existed as the seat of the region's ruler. We next hear of Jerusalem as a Jebusite stronghold which the Israelites could capture but from which they could not expel the inhabitants (Grabbe 2003a).

In early Iron I, settlements in the highlands suddenly blossomed. About the same time, the coastal plain was settled by a group whose material culture seems strongly influenced by Aegean and Cypriot forms (§3.2.2). Egyptian texts refer to the Sea Peoples who seem to have settled along the Palestinian coast. The much later biblical text refers to a variety of groups who inhabited the interior of Palestine, as well as the Israelites who are associated with the highlands in some passages. Although the biblical references are sometimes garbled or improbable, a number of the names coincide with peoples from Syria and Asia Minor (the Amorites, Hittites, Hurrians [Horites], Hivites, Girgashites), as well as the (indigenous?) Canaanites (§2.2.1.2; §2.2.1.5). On the whole, the biblical text is problematic for Iron I (and the earlier periods). Its picture of a massive exodus and rapid unitary conquest do not match either written records or archaeology, nor can much be found in the book of Judges that can be supported, even though the general scenario of a multitude of individual, (semi-)independent groups, each doing its own thing, seems to coincide broadly with what we know from other sources.

When 'Israel' next occurs in original sources (about the mid-ninth century BCE), it is now a kingdom in the northern part of Palestine, allied with the Aramaean king of Damascus and others against the expanding Assyrian empire (§4.2.1). Later Assyrian inscriptions make clear that alongside Israel to the south was another kingdom, that of Judah. What happened between the first mention of 'Israel' about 1200 BCE and the second mention some 350 years later? We have no information from primary written sources, only archaeology, plus we have the secondary source of the Bible which gives a detailed picture of the rise of a unified state under Saul, David and Solomon, followed by a split into the two kingdoms of Israel and Judah. Does this answer our question? Unfortunately, just as the exodus and conquest are contradicted by the primary sources, the biblical picture of the united monarchy has some problems associated with it.

The archaeology suggests that the inhabitants of the coastal plain were likely to be stronger in population and resources than those in the hill country. There is also the question of whether they would have felt the need to expand into the highlands at this stage. It is intrinsically unlikely that the highlanders would have conquered the Philistines at this point. The extensive conquests of David and the empire of Solomon from the 'river of Egypt'

to the Euphrates are not supported by the archaeology, the international context or the resources available. The visit of the 'queen of Sheba' simply has no support in the sources. The archaeology in Jerusalem so far does not corroborate a massive capital city with monumental architecture. There is still hope on the part of some that such will be discovered, but it seems unlikely that anything like the city envisaged in the Bible is going to be found. Archaeology is very important in all this, but the debate over the LC and ¹⁴C dating means that there is even less agreement than usual over what can be inferred from the material culture.

Some argue for a 'united monarchy' on a much reduced scale, with David perhaps more like a chieftain or the ruler of an archaic state of some sort. The arguments sometimes hinge on seeing David as an exceptional individual who was able to achieve this rule in spite of the expectations that the north would achieve statehood before Judah. Much also depends on being convinced that the biblical writer did not just invent it all – that some sort of historical scenario lies behind the text even if the compiler has relied on legendary material or exaggerated thinking. All this is, of course, possible. In 1998 I gave an address that made some predictions about the future of historical study (Grabbe 2000b). At that time, I suggested that the 'united monarchy' might not survive but did not predict that it would fall. Getting toward a decade later, the question still remains. The issue remains fiercely debated, but there is little new evidence. What seems overwhelmingly the case is that no one's idea of the 'united monarchy' bears much resemblance to the biblical description.

Although archaeology helps us bridge the gap between 1200 and 850 BCE, there is still much debate over how to interpret it and, therefore, much debate over how much of the text we can accept. But from early in the 'divided monarchy', we suddenly find aspects of the text supported fairly consistently by external sources: the names of Israelite and (later) Judahite kings, their relative time and order of reign, some of their deeds. The data confirmed tend to be those in portions of the text belonging to a particular literary formula that has long been associated with a court or temple chronicle. Sometimes two such chronicles (one from Israel and one from Judah) have been assumed, but I argue that the most parsimonious thesis is to hypothesize a single 'Chronicle of the Kings of Judah' (Grabbe 2006b). In some cases, in 1 and 2 Kings much of the king's reign seems to be drawn from such a chronicle, but in other cases (e.g., Ahab) we have other, more legendary, material taken from other sources (such as prophetic legends, in the case of Ahab).

The first kingdom for which we have solid evidence is the northern kingdom, the state founded by Omri. This fits what we would expect from

the *longue durée*; if there was an earlier state, we have no direct information on it except perhaps some memory in the biblical text. This does not mean that nothing existed before Omri in either the north or the south, but what was there was probably not a state as such. In any case, Israel is soon caught between the great empires to the north and south. Hardly has Omri passed from the scene before the Assyrians are threatening. In not much over a century, his kingdom had become an Assyrian province. Judah seems to have been the younger brother throughout their history and only came to flourish when Israel disappeared as a kingdom. But Judah, too, came under the Assyrian thumb even before Samaria fell. The seventh century was marked first by Assyrian domination, then Egyptian, then Babylonian.

As for our knowledge of Judah, we have the same type of records preserved that helped us with the northern kingdom: the same mixture of chronicle data, oral tales, legends and other material marks the descriptions of the reigns of kings until the fall of Jerusalem. In some cases, material has clearly been inserted for theological reasons, such as the accusations against Manasseh designed to blacken his name, and the claims that Josiah reformed the cult even as far as Samaria. Yet there is a gradual increase in reliability the later the narrative progresses. In the last years of the kings of Judah, when there are times that we know what was happening year by year, the biblical text can be remarkably accurate, and in the last few years (after 594 BCE) when our external sources cease, we can still have reasonable confidence that the basic narrative is correct. Yet plenty of inaccurate biblical text can also be found, such as the book of Daniel, which means that it must always be subject to critical analysis.

After Jerusalem fell in 587/586 BCE, there was no ‘empty land’ as was once proposed, but the settlement varied greatly. The population overall had been considerably reduced and was now concentrated to the north of Jerusalem in Benjamin, and Mizpah (Tell el-Nasbeh) seems to have been designated the administrative centre of the province by the Babylonians. Jerusalem appears to have been uninhabited or almost so during the four decades or so from the fall of Jerusalem until Persian rule.

This investigation has drawn attention to a number of broader points or themes relating to writing a history of Israel. By way of a conclusion to the study – and without repeating the general historical principles used throughout this study (§1.3.3) – here they are:

1. Writing the history of ancient Israel and Judah is no different from writing any other history. The data available and the problems involved may be different, but this is true of writing the history of any period or entity – each has certain unique problems and unique features. The

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problems with a history of the 'united monarchy' are very similar to those of a history of the Trojan War. The basic principle of giving preference to primary sources (as noted above) only goes so far, because much of what we want to know is not available in primary sources. Secondary sources (the biblical text and the Homeric poems) have to be considered.

2. The minimalist argument that the biblical text cannot be used except where there is external confirmation has often been supported in our investigation; however, as a working principle it is inadequate. The biblical text should always be considered: it is one of the sources for the history of ancient Israel and needs to be treated like any other source, being neither privileged nor rejected a priori, but handled straightforwardly and critically. Unfortunately, the Bible is not an 'ancient Near Eastern text' *tout court*: we know the biblical text solely from the versions (of which there are usually several) attested only in later copies. In some cases, these copies are mediaeval, though we are in the happy position that recent discoveries have allowed us to obtain versions of part of the text going back to the later centuries before the Common Era. Yet these finds show that the text was still developing, being edited, and growing until the first century CE.
3. Particular sorts of information are more likely to be trustworthy than others: wherever an Israelite or Judahite ruler is mentioned in an external source, the biblical text is shown to have the genuine name, the correct sequence of rule, and the approximate time of the person's rule in every case where there are sufficient external comparative sources to make a determination. Material likely to have been taken from or based on a court or temple chronicle has, *prima facie*, a much greater chance of having usable data. Prophetic stories, for example, do not generally make good historical sources. The main point here, however, is that the biblical text was not written as a record of the past nor for purely antiquarian reasons. Its purpose was a theological and religious one, as indicated already simply by the contents of the text. There is still a gulf between the concerns of the biblical writers and those of the historians beginning already with the ancient Greeks (cf. Grabbe 2001b). In general, though, the later a passage is in the history of Israel, the more reliable it is likely to be.
4. The most fruitful method is the multiple-source approach. Although preference should be given to archaeological and inscriptional sources, the use of a variety of data – archaeological, inscriptional, contemporary textual, biblical – has turned out to give us a reasonable grasp of some portions of the history of Israel, especially the later part of the

monarchy. It is important to study each source independently in the initial stages of the investigation, lest one 'contaminate' the evidence by circular interpretation of one against the other. Nevertheless, once the groundwork has been done and the nature of the different sources has been understood, then they can and should be synthesized in a rigorous way to work toward an understanding of the history of this period.

5. The social sciences can sometimes provide important models for and approaches to understanding the data. These models – theories – must always be critically tested against the data and not imposed on them. But they can be valuable in interrogating the textual and artifactual data available and suggesting new ways of interpreting them. The application of social anthropology to archaeology revolutionized things in the 1970s and 1980s, but texts may similarly acquire new significance by adding social scientific data and theoretical understanding to the intellectual process.

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